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ORIGINAL ARTICLES.

I.—Eminent Living Geologists: Henry H. Howell, F.G.S., formerly Director of the Geological Survey of Great Britain.

(WITH A PORTRAIT, PLATE XXI.)

AFTER a term of work extending over nearly half a century, Mr. Howell, the Director of the Geological Survey of Great Britain, has retired from the public service.

It is not often that a man who has only just reached the age-limit of 65 is able to look back on such a lengthened period of official work. No member of the Geological Survey has before attained such a record. Even Mr. Selwyn, whose portrait we gave in our February Number, had reached the age of 70 when he had nearly completed his fiftieth year of work on the Geological Surveys of Great Britain, Victoria, and Canada.

Henry Hyatt Howell was born on the 13th July, 1834, at Prinknash Park, Gloucestershire. He was the fifth son of Thomas Jones Howell, Esq., sometime Judge Advocate at Gibraltar, and Editor of the "State Trials." He was educated partly at the College School, Gloucester, and afterwards in the "Applied Science" Department at King's College, London, where at the time David Thomas Ansted, M.A., Fellow of Jesus College, Cambridge, was Professor of Geology.

In the spring-time of 1850, with an introduction from Colonel James, R.E. (afterwards Sir Henry James, and Director-General of the Ordnance Survey), he was attached (unpaid) to the surveying division of the Sappers and Miners then engaged at Wakefield, in Yorkshire, Captain Tucker, R.E., being in charge of that district. This survey was for the completion of the Ordnance Maps of the Six Northern Counties of England on the six-inch scale.

In the summer of the same year, with Lieutenant Cameron, R.E. (afterwards Director-General of the Ordnance Survey), he was attached to the surveying division then engaged in measuring the base line on Salisbury Plain with compensation bars.

Later on, in September of the same year, with a letter of introduction from Colonel James, he was permitted to join Prof. Ramsay,

then Local Director of the Geological Survey of Great Britain, at Llanberis, in North Wales, in order to gain some practical knowledge of geological surveying.

Thus when little more than sixteen years of age he commenced the active study of geology, and it was reported by Ramsay on September 11th, 1850, that at Carnedd Dafydd he "did a glorious day's work with Howell up to Aber, getting all Jukes's ugly bits of sandstone, etc., perfectly explained—a succession of domes cut off by faults. Home at half-past seven—a long, long walk."[1] A little later on young Howell spent a short time with Beete Jukes on the same Survey at Llangollen.

On the 7th October Howell was appointed an Assistant-Geologist "upon the usual course in our service of a six months trial." He, however, gave so much satisfaction in the field while with Professor Ramsay and Mr. Beete Jukes, that the time of probation was shortened to three months, and the appointment was confirmed in a letter dated 1st January, 1851. This was written by Sir Henry De la Beche, who was then Director-General, so that the last link which connected the present staff with the illustrious founder of the Geological Survey is now officially severed.

Aveline, Bristow, Selwyn, Edward Forbes, Salter, and Smyth were then on the staff. Hull had also but recently joined. During his three months of probation Howell was placed at Rugeley with Beete Jukes, who was then engaged in mapping the northern part of the South Staffordshire Coalfield. No more excellent a guide could he have had than Jukes, whose wide experience, ability, and sound judgment made him an excellent teacher, and whose field-work has been a model to all subsequent observers. It was also at this time that Howell was instructed to complete the survey of the one-inch quarter-sheet 72 S.E., which included the outliers of 'Lias' (now known to be the Rhætic Beds) on Needwood Forest.

For several years Mr. Howell was occupied in the survey of the Midland Counties in the neighbourhood of Lichfield, Tamworth, Atherstone, Nuneaton, Coventry, Birmingham, and Droitwich. He also mapped parts of North Gloucestershire, his notes being published in the Geological Survey memoir on "The Geology of the Country around Cheltenham" (1857), by E. Hull, who states that "The districts adjoining the Severn and Avon, together with Bredon Hill and the northern part of the Cotteswold Range as far south as the road between Bourton-on-the-Hill and Snowshill, have been surveyed and described in this memoir by Mr. Howell."

Probably the most important work done at this period (between 1851 and 1854) was the survey on the one-inch scale of the Coalfield of Warwickshire. Howell's memoir on "The Geology of the Warwickshire Coalfield and the Permian and Trias of the surrounding district" was published in 1859, and this gives a concise and excellent account of the Coal-measures and overlying strata. Some changes have since been made in the grouping of the older

[1] Geikie's Memoir of Ramsay, p. 167.

rocks, the Hartshill quartzite having at the time been regarded as Millstone Grit. This view was not arrived at without considerable discussion. The rock had originally been compared with that of the Bromsgrove Lickey, and both the late Professor Jukes and Mr. Howell were in favour of retaining the beds in the then very comprehensive "Silurian series,"[1] but Sir A. Ramsay (at that time Local Director) was of opinion that the quartzite and overlying shales should be included with the Carboniferous. The subsequent researches of Professor Lapworth and Mr. W. J. Harrison (1882) have shown that the quartzite is of Cambrian age,[2] thus generally confirming the view of Jukes and Howell of the antiquity of the quartzite.

Apart from the interesting but often arduous work of mapping there was much other work to be done: the districts surveyed by Mr. Howell were illustrated by many sheets of vertical sections, and also of 'horizontal' or longitudinal sections drawn to a natural scale of six inches to a mile. The task of producing these longitudinal sections was in old days a tedious and troublesome one, requiring great care and skill. Nowadays with the contoured six-inch maps the surveyor can readily plot his sections in the office. Then he had to traverse the ground with theodolite and chain, and to conduct his operations with the aid of a couple of men.

In many instances the horizontal sections were accompanied by short descriptive pamphlets (price 2*d.*), and in these (e.g., Explanations of Sheets 46, 48, 50, 51, and 57—all published in 1859) we find many notes on the strata examined and mapped by Mr. Howell. He was also joint author with Mr. Aveline of a short memoir on "The Geology of part of Leicestershire" (1860).

In the autumn of 1854 the Geological Survey of Scotland was commenced by Ramsay in the neighbourhood of Dunbar. His Directorial duties, however, prevented him from accomplishing very much field-work, and in consequence Mr. Howell was transferred to Scotland, and commenced work there in 1855. In course of time he surveyed large areas in the counties of Berwick, Haddington, and Edinburgh, his chief work being the mapping of the Mid and East Lothian and Fifeshire Coalfields.

In the same year (1855) Mr. Howell was joined by Mr. (now Sir) Archibald Geikie (the present Director-General), who, "after some months of field-work with Mr. Howell in continuing the Director's mapping in East Lothian, began the survey of Midlothian in 1856."[3] A joint work on "The Geology of the Neighbourhood of Edinburgh," by H. H. Howell and Archibald Geikie, was published in 1861. The descriptions of the Midlothian Coalfield and chapters on the Millstone Grit and Coal-measures were written by Mr. Howell. Meanwhile, in 1857, Mr. Howell had been promoted to the rank of Geologist. Later on (in 1861) John Young, M.D. (now Professor

[1] See Strahan, GEOL. MAG., 1886, p. 541.
[2] Lapworth, ibid., 1882, p. 564.
[3] Geikie's Memoir of Ramsay, p. 244.

of Natural History in the University of Glasgow) joined the staff, and he aided the field-work in Scotland. In 1866 the memoir on "The Geology of East Lothian," by H. H. Howell, A. Geikie, and John Young, was issued. Several maps on the six-inch scale of the coalfields of Edinburgh and Haddington were also published.

The main geological result of the work in these Carboniferous regions of Scotland was the recognition for the first time that the "Roslin Sandstone" was the representative of the Millstone Grit, and occupied the same position as does that formation in the Carboniferous Series of England and Wales. Moreover, it was ascertained that the true Coal-measures, with many workable seams of coal, occurred, as in the Northumberland and Durham Coalfield, at a higher horizon than the Millstone Grit. The same group of rocks as had been found in East and Mid Lothian was shown to extend across the Firth of Forth into Fifeshire.

In 1861 Mr. Howell was transferred again to England, and for the next four years he was occupied in mapping (on the one-inch scale) large areas of Jurassic rocks, chiefly in the counties of Northampton, Bedford, and Huntingdon.

The continuation of this work into Rutlandshire and South Lincolnshire was in after years carried on by Professor Judd, who, together with the late Samuel Sharp, ascertained that the Lincolnshire Limestone, which had been regarded as Great Oolite, was of the age of the Inferior Oolite. On a memorable occasion in 1869 the revised interpretation of the geology was considered by the Director, Ramsay, who visited parts of Northamptonshire with Howell, Judd, Sharp, and Etheridge.

Ramsay, after reviewing the evidence, turned, in his quick impatient way, to Howell to ask why he had regarded the Limestone as Great Oolite? To which Howell replied by quietly pulling from his breast-pocket a list of the fossils which he had collected, and which had been named and referred, in the Palæontological Office, to the *Great Oolite.*

In 1866 Mr. Howell was instructed to commence the geological survey of the Northumberland and Durham Coalfield.

In the following year, when large additions were made to the staff of the Survey, Mr. Howell took the superintendence of a number of the men who were engaged in the Survey of the North-Eastern Counties. At this time the late Prof. A. H. Green took immediate charge of the Yorkshire Coalfield, while in course of time Mr. Howell directed the whole of the work in North Lincolnshire, East Yorkshire, Durham, and Northumberland. In 1869 he was called upon to give evidence on the Northumberland and Durham Coalfield before the Royal Commissioners appointed to enquire into the several matters relating to Coal in the United Kingdom.[1] In 1872 he was promoted to be District Surveyor, the duties of which office he had in fact for a long time been discharging.

[1] See Report, vol. ii (1871), p. 503.

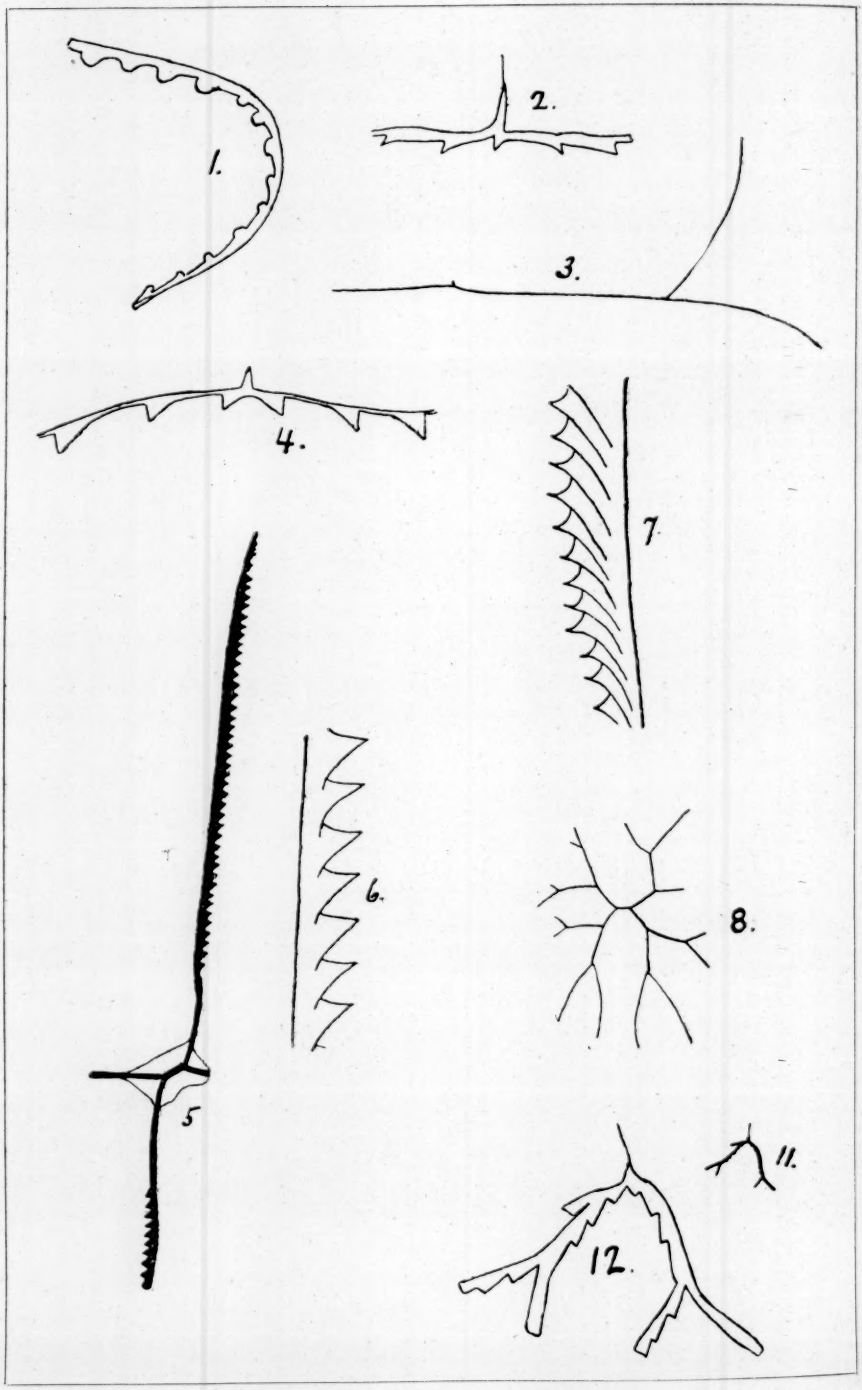
For about sixteen years Mr. Howell was thus busily occupied, partly in carrying on field-work himself, more largely in superintending the work of the staff placed under him. With a number of men of varied ability, energy, and enthusiasm, it is a well-nigh impossible task to carry on the field-work in a strictly uniform manner. The methodical habits and the stern sense of duty inculcated by Mr. Howell proved, however, of inestimable value in the conduct of the Survey, and it may be candidly admitted that no large tract of ground had before been so systematically surveyed and inspected on the Geological Survey. As a field-geologist and administrator no man came to be more highly esteemed by the members of the Survey who had worked under him.

On the retirement of Ramsay in 1882, and when the present Director-General was appointed in his stead, Mr. Howell was made Director for Scotland. About this time several of the more experienced surveyors, who had worked under Mr. Howell in the North of England, were transferred to Scotland. A great change in the methods of mapping was then introduced—changes which have borne excellent fruit in the detailed surveys which have since been carried out in the highlands of Scotland.

When the late H. W. Bristow retired in 1888, Mr. Howell was appointed Director for Great Britain, although his special sphere of action did not extend beyond the northern counties of England, the detailed supervision of the men in the midland and southern parts of England and Wales being relegated to senior geologists. During these later years the mapping of the Isle of Man was commenced and completed under the superintendence of Mr. Howell.

Mr. Howell has added little to the general literature of geology. We have referred to his official labours in maps and sections, in memoirs and explanatory pamphlets, but we have no extended list to add of papers communicated to scientific societies and magazines. We may, indeed, mention one paper published in this Magazine (for 1890, pp. 8–13) entitled "Note on the Classification of the Red Rocks in South-East Durham; and on a possible unconformity between the Trias and the Permian Limestone in the same District."

The fact is that Mr. Howell has given his life to official work, in later years almost wholly to those administrative duties which bring but little praise or reward to the individual, except so far as his guiding hand and advice are seen in the good work accomplished by others. His life has been one of disinterested devotion to the welfare of the public service, and of the men who have had the privilege of working under his direction. We cordially wish him all happiness in the days of his retirement.



Graptolites of Victoria, Australia.

II.—The Graptolite-bearing Rocks of Victoria, Australia.

By T. S. Hall, M.A., Melbourne University.

(PLATE XXII.)

Introduction.

THE area occupied by Lower Palæozoic rocks in Victoria is an extensive one, and is pretty equally divided between Ordovician and Silurian, the part occupied by Cambrian being small. According to Dr. A. R. C. Selwyn, formerly Director of the Geological Survey of the Colony, the total area amounts to somewhere about 30,000 square miles in extent,[1] and although in this estimate he included all areas occupied by these rocks provided the cover was not more than 350 feet in thickness, it will still leave us with a large extent of country over which graptolites may be found.

The earliest record of the occurrence of these fossils in Victoria is on a map of Selwyn's published in 1856, where it is stated in a note that graptolites were found for the first time in Australia by C. D. H. Aplin in May, 1856, at a locality indicated on the Saltwater River near Keilor, about ten miles north-west of Melbourne. No identifications have ever been made from these beds, which yield a few species of Monograptidæ in a poor state of preservation.

Very shortly after this graptolites were found in the Lower Ordovician rocks of Bendigo. In an essay contributed to the Catalogue of the Victorian Exhibition of 1861, Professor McCoy recorded from various localities several species which included Diplograptidæ, Dichograptidæ, and Dicranograptidæ, as well as *Monograptus ludensis*, which showed that even thus early almost the whole range of graptolite-bearing rocks had been examined.

In a similar essay by the same author published in 1867, a few additional forms were noted.

The first decade of Sir F. McCoy's "Prodromus of the Palæontology of Victoria" appeared in 1874, and contains the earliest figures and descriptions by which we can judge of the accuracy of his previously published determinations. The plates seem to have been printed off several years previously, for Professor James Hall, in his "Graptolites of the Quebec Group," says that he received a plate from Professor McCoy in 1861, and which from his remarks would appear to be one which was not published till 1875. Two years later, in his fifth decade, Professor McCoy issued figures and descriptions of a few more of these fossils. In 1874 Mr. R. Etheridge, jun., published figures and descriptions of several forms which he identified with known species, the date of his paper in the *Annals of Natural History* and of McCoy's first decade being almost simultaneous.

With regard to the determinations contained in these and other early papers, it may be stated generally that, as was the case of

[1] Intercolonial Exhibition Essays, 1866–7: "Notes on the Physical Geography, Geology, and Mineralogy of Victoria," p. 13.

many of those of Britain before Professor Lapworth undertook his work of revision, they are in very many cases unreliable. The causes that led to this in the British records have been clearly enunciated by Professor Lapworth, and though it must always appear ungracious to reject any of the work of the pioneers of Australian palæontology, to whom we owe so much, yet the ground must be cleared before a stable structure can be raised. The minute differences on which it has been found advisable to separate the species in this difficult group were not then generally recognized, and we find many of our graptolites identified with forms from which we now regard them as even, it may be, generically distinct. In the case of those forms where the method of branching and the habit is a guide there was, of course, less liability to confusion, and here the specific identifications are of value, but it is extremely doubtful, on the other hand, whether any of the Diplograptidæ have been correctly determined, and a great number of those forms referred to *Didymograptus* (*sensu stricto*) are probably incorrectly identified. One feature, however, must not be overlooked, and this is that the records have in many cases been made from exact localities; and this in the case of Sir F. McCoy's papers, owing to his official connection with the Survey, is of peculiar value, as the precise position from which the fossils came is recorded both by him and on the geological maps, and we are thus frequently enabled to check the records in a very effective way.

During the past seven years I have published several papers on the group as represented in Victoria, and two have also appeared by Mr. G. B. Pritchard, most of them having been printed in the Proceedings of the Royal Society of Victoria.

In a paper dealing with the occurrence of Lower Silurian (i.e. Ordovician) graptolites in New South Wales, Mr. W. S. Dun[1] gives a list of the more important Australian literature referring to the group.

Sequence.

The general sequence of the Victorian graptolites may be correlated with that of the Northern Hemisphere, but experience has shown that it is unsafe to push the analogy too far, and that the only safe method is that of detailed stratigraphical work. Thus we find forms here associated which elsewhere are separated by intervening zones; and, on the other hand, forms elsewhere associated may be here separated. A form which I have named *Leptograptus antiquus*,[2] and which, though not perhaps a typical member of the genus, is certainly not a *Didymograptus*, occurs on the same slabs as a *Bryograptus*. *Didymograptus bifidus*, again, which in Europe and America is characteristic of Upper Arenig, dies out in Australia long before *Phyllograptus typus* has disappeared, and is also survived by a *Clonograptus* and two or three species of *Dichograptus*. At Lancefield, where our lowest Ordovician rocks occur, *Clonograptus flexilis* and

[1] Records of the Geological Survey of New South Wales, vol. v (1897).
[2] Proc. Roy. Soc. Victoria, new ser., vol. xi (1899), p. 164.

C. rigidus are found in association with *Bryograptus*, while in Europe and America *Bryograptus* is a Cambrian form, and indicates an age to which its Victorian associates forbid us to refer it here. In the Northern Hemisphere, again, *Clonograptus flexilis* is associated with forms which characterize the next higher horizon with us. The case of *C. rigidus* is also striking, for though in America it is, according to Ami,[1] associated with *Loganograptus*, yet with us the latter genus does not appear till *Phyllograptus typus* and closely allied species have become extinct. These are all striking forms, and although it is of course not impossible that mistakes in identification have been made, yet figures and descriptions of Australian forms referred to these species have been published by McCoy and myself, with the exception of the members of the tuning-fork graptolites.

The stratigraphical evidence on which these statements are founded does not rest on merely a single set of outcrops, for, with the exception of the Lancefield beds, the field relations of which have not yet been worked out, as the conditions are unfavourable, their relative position has been studied in two or three places.

To prove a negative is, of course, not an easy task, and it may be objected that the non-association of some of the forms I have quoted is only due to imperfect collecting. For instance, I have placed the Castlemaine rocks as a whole above those of Bendigo, and both above those of Lancefield, and have never seen *Clonograptus flexilis* anywhere but at Lancefield, where *C. rigidus* occurs as well. A species related to the latter, which, however, I believe to be distinct, ranges up into the lower Castlemaine zones, but never as high as beds which contain *Loganograptus Logani*. This statement is founded on several years' collecting in fairly rich beds at Castlemaine and Bendigo, as well as on the examination of several collections made at the latter place by others, so that the evidence in favour of the facts being as I believe them to be is strong.

The numerous localities at which graptolites occur in Victoria and the vast area over which they are spread render the hope of anything like a complete account of them for some time to come quite out of the question, and all that can be done at present is to give a brief outline of what has been done towards elucidating their sequence. There is not at present sufficient evidence available to do more with the Ordovician rocks as a whole than to divide them into Upper and Lower, the upper division being characterized by the presence of Dicranograptidæ.

The most thorough collecting that has been done is in the older beds of the Lower Ordovician, and even here large areas are almost untouched. The younger beds of the series have not as yet yielded many forms in a sufficiently well-preserved condition for description, while those from the Upper Ordovician occur at widely scattered localities, and the collections made are small and poor. Still worse is the case of the Silurian forms, for they come from some half-a-dozen localities and afford little more than a record of *Monograptus* and *Retiolites*.

[1] Geol. Surv. Canada, Ann. Rep., vol. iii (1887-8), K, p. 116.

Main Divisions of the Graptolite Beds.

Lower Ordovician.

1. *Lancefield Series.*—The blue black shales of this locality yield a fauna which I have dealt with in some detail elsewhere.[1]

The species recorded are: *Bryograptus Victoriæ*, T. S. Hall; *Bryograptus Clarki*, T. S. Hall; *Leptograptus antiquus*, T. S. Hall; *Didymograptus Pritchardi*, T. S. Hall; *Didymograptus Taylori*, T. S. Hall; *Tetragraptus decipiens*, T. S. Hall; *Clonograptus flexilis*, J. Hall; *Clonograptus magnificus*, Pritchard; *Clonograptus rigidus*, J. Hall; *Clonograptus rigidus*, var. *tenellus*, Linnars; *Phyllograptus*? sp.; *Dictyonema Macgillivrayi*, T. S. Hall; *Dictyonema pulchellum*, T. S. Hall.

There is at first sight a strange intermingling of genera here, but the species are all associated in one small excavation, and no separation of the beds is possible. From the occurrence of *Bryograptus*, which is fairly common, the beds must be regarded as the oldest of our graptolite-bearing rocks yet found, while from the presence of the other associated genera they cannot, I think, be regarded as Cambrian, but must be looked on as Ordovician.

2. *Bendigo Series.*—As far as is known, the greatest development of these beds is in the Bendigo district, where our richest auriferous quartz veins occur. A similar fauna is also found at Chewton, Daylesford, Tarilta, Newstead, Spring Plains, Upper Loddon, and near Lancefield.

Minor subdivisions in this thick series will doubtless be made, but at present no salient differences have been noted between the faunas of the various exposures. The fauna is rich, and at Bendigo is usually fairly well preserved, though the beds are often indurated and much cleaved. Many new species are as yet undiagnosed, and several of the old records of *Didymograptus* (*sensu stricto*) are probably incorrect. I have not seen any Diplograptidæ from the rocks of this series, though two species have been recorded from Bendigo. As the family is represented in younger rocks a few miles to the north of the town, in what is known as the Whipstick Country, it is probable that they came from there, the term Bendigo having been used in a loose geographical way. In any case the specific record is open to doubt.

The following species occur, amongst others: *Didymograptus bifidus*, J. Hall; *D. gracilis*, Tqt.; *D.* cf. *decens*, Tqt.; *D. caduceus*, Salter; *Tetragraptus fruticosus*, J. Hall; *T. serra*, Brong.; *T. quadribrachiatus*, J. Hall; *Dichograptus octobrachiatus*, J. Hall; *Phyllograptus typus*, J. Hall; *Goniograptus Thureaui*, McCoy; *G. macer*, n.sp. (vide infra).

A mere inspection of the list hardly brings out the character of the fauna, as a good deal depends on the relative abundance of certain forms. *Tetragraptus fruticosus* and *Phyllograptus typus* are very abundant, while *Didymograptus bifidus* is very rare and

[1] Proc. Roy. Soc. Victoria, new ser., vol. xi, 1898 (1899), pp. 164–178, pls. xvii, xviii, xix.

perhaps indicative of the higher beds of the series. *Didymograptus caduceus*, again, is rare and small, and it is interesting to notice, as we pass up through a long series of rocks above those of Bendigo, that it increases in relative numbers, and at the same time gradually attains a much larger size, till it reaches its maximum near the horizon of the uppermost Castlemaine beds, where it crowds the rocks to the almost entire exclusion of other forms. It then enters on the period of its decline, is but sparingly represented by stunted forms at Darriwill, and perhaps ranges up into the Upper Ordovician.

3. *Castlemaine Series.*—The rocks which form this series overlie the Bendigo beds conformably. The change in fauna is slight, but *Tetragraptus fruticosus*, so characteristic of the older beds, is absent, and *Didymograptus bifidus* becomes abundant at the base of the Castlemaine rocks. I have subdivided the rocks into several minor zones,[1] but need not here consider the details. The summit of the series has not been clearly marked off from the beds which succeed it, as they are not known to come into contact.

In the lowermost beds *Didymograptus bifidus*, J. Hall, as just stated, is abundant, but soon disappears. *Phyllograptus typus* long persists, and is survived for a short time by *P. angustifolius*, J. Hall. Species of *Diplograptus* appear in the higher beds, but not, apparently, in the lower. A species of *Clonograptus* occurs in the lower beds, but soon disappears. *Loganograptus Logani* puts in an appearance in the highest zones and ranges up into the Darriwill Series.

Though the position of *Didymograptus bifidus* and its allies would appear to be somewhat lower, relatively, than what it is in the Northern Hemisphere, yet if it be true, as has been suggested by Nicholson and Marr, that the 'tuning-fork' group is phylogenetically connected with *Tetragraptus fruticosus*, then its position in beds so closely associated with those in which the latter species occurs is no more than we should reasonably expect to find. The really striking point is that it should die out so soon with us, and characterize rocks that are closely connected by their fauna with the Lower Arenig and not with the Upper. Although we seem to be able to trace the descent in a general way of the 'tuning-fork' group from the *Tetragraptus fruticosus* group, still we are unable, I venture to think, to descend from generals to particulars.

The late appearance of *Loganograptus* is of interest. As it is one of the species figured by McCoy in his "Prodromus of the Palæontology of Victoria," and was stated to occur commonly at Castlemaine, I was naturally on the look out for it, and for a long time in vain. I found it at last about a mile to the south of the town, where I made a prolonged search for it, on the strike of McCoy's locality, which is in the centre of the town and inaccessible. It is never associated with *Phyllograptus typus*, which does not range as high as the beds in which *Loganograptus* first appears. *Dichograptus octobrachiatus* has also disappeared, after having long

[1] Proc. Roy. Soc. Victoria, new ser., vol. vii (1894), pp. 55–88.

persisted, while the commonest associate is *Didymograptus caduceus*, which is represented by the large variety characteristic of the higher beds of the Castlemaine Series. In Canada the only species given by Ami[1] as associated with it are *Rouvilligraptus Richardsoni* and *Goniograptus Thureaui*. The former species has not been recognized in Australia, while the latter is confined to a lower horizon, not passing up into the Castlemaine Series. In the Skiddaw Slates it would also appear that the position of *Loganograptus* is lower in the series, for Miss G. L. Elles,[2] in her table of suggested phylogeny, puts down *Dichograptus octobrachiatus* and *Tetragraptus quadribrachiatus* as derived from *Loganograptus Logani*. Genealogical trees are notoriously difficult to draw up in biology where there is great danger of confusing the different collateral branches. The question suggests itself as to what is the real order of the appearance of these forms. Migration, one would think, must produce apparent inversions in widely separated regions, and it will be evident that several such inversions actually do occur in the case of our graptolites, or, in other words, that we find in Victoria forms associated which are not found together in the Northern Hemisphere, and *vice versâ*. That the general phylogeny of the graptolites may be traced out on the lines indicated by Nicholson and Marr,[3] and so ably elaborated by Miss Elles, there can be but little question, but the task of elaborating the details is complicated when we take into consideration the possibility that the succession of life in any particular set of beds may not be the true one. The general succession of the graptolites in Australia and the Northern Hemisphere is the same, but there are undoubtedly differences in details.

The Castlemaine Series has been best studied in the district whence its name is taken, and it there includes, as far as I have seen, all the beds above the Bendigo Series, which is but slightly exposed on the main anticline which passes through Chewton.

The group includes rocks occurring at Castlemaine, a large area in the Bacchus Marsh and Coimaidai Districts,[4] Gisborne, and perhaps a considerable part of the intervening country, as well as an area in the Whipstick, north of Bendigo.

The fauna includes *Didymograptus bifidus*, J. Hall; *D. Murchisoni*, Beck; *D.* cf. *decens*, Tqt.; *D. extensus*, J. Hall; *D. caduceus*, Salter; *Tetragraptus serra*, J. Hall; *T. quadribrachiatus*, J. Hall; *T. proiectus*, n.sp. (v. infra); *Dichograptus octobrachiatus*, J. Hall; *D. octonarius*, J. Hall; *Clonograptus*, sp.; *Phyllograptus typus*, J. Hall; *P. angustifolius*, J. Hall; *Loganograptus Logani*, J. Hall; *Goniograptus macer*, n.sp. (v. infra); *Dendrograptus*, sp.; *Diplograptus*, spp.; *Climacograptus*, sp.; and perhaps *Trigonograptus*, as well as many forms as yet unidentified.

4. *Darriwill Series.*—The only locality at which these rocks are known is in the parish of Darriwill, where graptolites were found by

[1] Geol. and Nat. Hist. Surv. Canada, new ser., Ann. Rep., vol. iii, pt. 2, K, p. 116.
[2] Q.J.G.S., vol. liv (1898), p. 537.
[3] Geol. Mag., 1895, pp. 529–539.
[4] Proc. Roy. Soc. Victoria, new ser., vol. x, 1897 (1898), pp. 202–3.

Wilkinson in 1864. The fauna differs from the typical Castlemaine one by the almost entire absence of *Didymograptus caduceus*, and the appearance of *Lasiograptus* and *Glossograptus*. *Trigonograptus* and several species of *Didymograptus* and *Climacograptus* occur, while *Tetragraptus serra* and *Loganograptus* still persist. Sir F. McCoy records *Tetragraptus Headi*, but judging by the figure and description I am inclined to doubt the identification.

The beds are very rich, and many new forms are present, but the rocks at the only fossiliferous localities known are so much weathered that further elucidation of the fauna is rendered difficult.

This series appears to mark the close of the Lower Ordovician, as in the succeeding rocks Dicranograptidæ put in an appearance.

Upper Ordovician.

No stratigraphical work has yet been done in the Upper Ordovician beds, so that in the present state of our knowledge it will be unwise to attempt any correlation based on the occurrence of a few stray forms the vertical range of which we cannot positively state.

Rocks rich in graptolites, but in a very bad state of preservation, occur on the Saltwater River about twenty miles north of Melbourne. There are present *Cœnograptus gracilis*, apparently, as well as *Dicranograptus ramosus* and several species of *Dicellograptus* and of Diplograptidæ.

A somewhat similar fauna has recently been found near Matlock on the Woods' Point Road,[1] whence I have identified *Diplograptus foliaceus*, Murch., and perhaps *Dicellograptus Morrisii*, Hopk. Diplograptidæ are also represented. The rocks of this area have long been considered Silurian from the occurrence of *Cardium Gippslandicum*, McCoy, the only fossil known from the district. Elsewhere this *Cardium* is associated with an *Orthoceras*, identified by McCoy as *O. striatopunctatum*, Münst., and so considered by him to be of Silurian age. As the age of the graptolites is Ordovician beyond a doubt, the fact of the occurrence of Silurian rocks in the neighbourhood may be questioned till better evidence is forthcoming, though of course the Ordovician may be a small inlier.

The age of the rocks in North-Eastern Victoria was long in doubt, but the discovery by Mr. Ferguson[2] of what proved to be Upper Ordovician graptolites at three widely separated localities showed that a great part of the area must be occupied by rocks of that age. I have identified *Dicellograptus elegans*, Carr., and *Climacograptus bicornis*, J. Hall, from Wombat Creek, where Diplograptidæ are also represented. At Tungamah we get *Diplograptus pristis*, His., and perhaps *Dicellograptus sextans* and *Dicellograptus ramosus*, all, however, badly preserved. From Walwa Creek come *Dicellograptus anceps*, Nich.; *Diplograptus pristis*, His.; *D. truncatus*, Lapw.; *Climacograptus bicornis*, J. Hall. From Eastern Gippsland a few forms, also belonging to the Upper

[1] Proc. Roy. Soc. Victoria, new ser., vol. x (1897), pp. 13–15.
[2] Id., vol. x, 1896 (1897), pp. 183–6.

Ordovician, have been doubtfully recorded,[1] the condition of the specimens rendering specific identification impossible, though I consider the generic character of the fossils sufficiently clear.

Silurian.

The Saltwater River, in its course south from the Dividing Range, passes over the boundary between the Ordovician and Silurian formations. The river-valley is cut deeply through the great basaltic plain of Western Victoria, which here reaches almost its eastern limit. As is usual in such gorges, the basaltic detritus deeply masks the hill-sides, and outcrops of older rocks are isolated and small, so that whether the junction is conformable or not has not been settled. The question has been avoided on our geological maps by colouring Ordovician to the border of one sheet and beginning the next with Silurian. In the north-east of the colony, however, at Wombat Creek, there is, according to Mr. W. H. Ferguson, of the Department of Mines, an unconformable junction with a conglomerate at the base of the Silurian. The graptolites from the lower beds which he obtained, and which were submitted to me by the Department, are, as mentioned above, of Ordovician age. The fossils from the upper series over the conglomerate were sent to Mr. R. Etheridge, jun. They were in a very imperfect state, but from an examination of them Mr. Etheridge says [2] that he is "inclined to regard the specimens . . . . as of Upper Silurian age," Upper Silurian being used in the sense in which Silurian is used in this paper. There would appear from this, then, that there is a physical unconformity between the two formations in Victoria. That an unconformity exists was long ago asserted by Dr. Selwyn,[3] the statement being based on the difference in the average amount of dip in the two formations, and not on any observed contact. Mr. R. A. F. Murray, lately Government Geologist of the Colony, says that "the precise lines of junction of the two groups have not yet been so nearly ascertained as to enable it to be stated that they are stratigraphically unconformable."[4] Mr. Ferguson's observations, however, would seem to settle the point.

As previously mentioned, the earliest known graptolite locality in Australia lies near the township of Keilor, where Aplin found specimens in 1855. There are one or two species of Monograptidæ present, none of which I find well enough preserved for description, and *Diplograptus* is possibly represented. About a mile from the locality just mentioned another outcrop occurs, which contains a few examples of *Monograptus*, but which is chiefly noticeable as being the spot where *Retiolites Australis*, McCoy, is found, and which has not hitherto been found elsewhere.

The records from other localities are not very satisfactory. I have identified *Monograptus priodon*, from Macclesfield, and *M.* cf. *dubius*,

[1] Geol. Surv. Victoria, Prog. Rep., vol. ix (1898), pp. 126–8.
[2] Ibid., p. 126.
[3] Intercolonial Exhibition Essays, Melbourne, 1867, p. 12.
[4] "Geology and Physical Geography of Victoria," p. 43.

Suess, from South Yarra, while a few other badly preserved examples of *Monograptus* have also been found at this last locality. From Alexandra a large number of specimens of the same genus have been obtained, all apparently of the one species, but all so badly preserved that no useful purpose would be served by either recording them as old species or describing them as new. It is worthy of remark, however, that no sign of Diplograptidæ has been found at this locality, so that we may perhaps assume that the beds are not very near the base of the formation.

The record, then, of Silurian forms is meagre in the extreme, for we can do little more than assert their presence in the formation, and must await the advent of better material before reliable definite statements can be made.

The absence of many characteristic genera, both Ordovician and Silurian, from our records must be regarded as apparent rather than real. The Lower Ordovician beds are the only ones that have been examined in any detail, while the Upper Ordovician rocks, hitherto worked, are either badly jointed mudstones or highly indurated slates, in which graptolites merely appear as a glaze on the surface.

Other Australasian Localities.

With regard to other Australasian graptolites the information is as yet meagre.

Tasmania has apparently yielded a single specimen, which was identified as *Diplograptus nodosus*.[1] I have elsewhere[2] given reason for supposing that a *Diplograptus* was found, and for the worthlessness of the specific identification, and for the correlation with Victorian beds founded upon it.

New South Wales has recently afforded some Upper Ordovician forms as recorded by Mr. W. S. Dun.[3] Dicranograptidæ and Diplograptidæ are common, but badly preserved. *Phyllograptus* is doubtfully recorded from the same beds, and Mr. Dun kindly showed me the specimen, which, however, I am inclined to regard as one of the Retiolitidæ. *Phyllograptus* certainly does not range as high. These forms come from the south-east corner of the colony, but similar forms have since then been found as far north as the latitude of Sydney, and throw considerable light on the age of a series of slates and sandstones hitherto tentatively regarded as Silurian.

In New Zealand Lower Ordovician graptolites occur, and Sir James Hector[4] gives a few rough woodcuts of some of the forms, but does not identify any of the species. Frech[5] gives a few records, which are all of Lower Ordovician forms, and I have examples of *Didymograptus bifidus* from Nelson.

[1] Thureau, "Report on the . . . Lisle Goldfield": Tasmanian House of Assembly Journal, vol. xliii (1882), No. 146.

[2] Rep. Aust. Ass. Adv. Science, Sydney, 1898, p. 401.

[3] Records Geol. Surv. New South Wales, vol. v (1897), p. 124.

[4] Cat. N.Z. Geol. Exhibits, Ind. and Col. Exhib., 1886, p. 82.

[5] Lethæa Palæozoica, Bd. i (1897).

Remarks on some of the Forms, with Descriptions of New Species.

Monograptus, sp. (Pl. XXII, Fig. 1.)

The specimen figured shows the characters of the genus, but is too imperfect for specific description, and the figure is merely given as evidence of the occurrence of the genus in our beds.

Locality. — Keilor, from the locality where Aplin first found graptolites.

Didymograptus gracilis, Törnquist. (Pl. XXII, Fig. 2.)

Acta Univ. Lund, vol. xxvi, pt. 2, iv, p. 17, pl. i, figs. 9–12.

Hydrosome slender. Sicula about 1·3 mm. long and very narrow, with a very delicate virgula, which is traceable for about 0·7 mm. Branches extending at 180° and apparently arising at very different levels from the sicula, the left side of which projects as an acute tooth. The left branch is given off from about its middle, the right branch appearing to rise at the level of the aperture. Dorsal edge of the branches convexly swollen opposite the middle of each theca. Thecæ about seven in 10 mm.; slender; outer edge slightly concave, apertural edge straight and facing somewhat inwards, the two edges forming an acute denticle.

Törnquist says that he had an example showing the hair-like virgula, but that he was subsequently unable to find it.[1] It is clearly visible in the figured specimen. Breadth of branch about 0 3 mm.; width of thecæ at aperture about the same.

Locality.—Bendigo; exact locality unknown.

Leptograptus antiquus, T. S. Hall. (Pl. XXII, Figs. 3, 4.)

Locality.—Lancefield.

Tetragraptus projectus, n.sp. (Pl. XXII, Figs. 5, 6.)

Primaries in a straight line, about 1 mm. long. Secondaries diverging at an angle of 90° from each other, rigid, narrow, about 1·5 mm. broad, and may be more than 6 cm. long. Central disc oblong, with straight sides, slightly curving outwards at the angles to embrace the secondary branches. Length parallel to the direction of the primary branches, 7 mm.; breadth, 5 mm. Sicula not visible. Thecæ eight in 1 cm.; short; slightly expanding; apertural margin slightly concave, forming an angle of about 105°; outer margin concave, inclined at 50° to 55°. Amount of overlap not clearly shown in the only specimen found, and cannot be traced further than is shown in the enlarged figure (6).

The narrowness of the secondary branches and the prominent thecæ distinguish the species from all others to which I can find reference.

Locality.—A single specimen, both sides preserved, from the slate quarries five miles west of Gisborne, associated with the large form of *Didymograptus caduceus*, *Tetragraptus serra*, *Diplograptus*, sp., *Climacograptus*, sp., and apparently *Phyllograptus typus*.

[1] Loc. cit., p. 17, footnote.

Dichograptus octonarius, J. Hall. (Pl. XXII, Fig. 7.)

The tertiary branches at a little more than 1 cm. from the sicula are from 2·5 to 3 mm. broad. Thecæ eleven in 1 cm., slightly expanding, curved, inclined at about 30° in the middle part of their length, and at nearly twice that angle near their aperture. Apertural margin strongly concave, forming an angle of about 150°, and making with the outer margin a prominent denticle. The species seems somewhat more lax in habit than *D. octobrachiatus*, and specimens are common in the middle beds of the Castlemaine district, which lie in very similar positions to that figured by Hall in his "Graptolites of the Quebec Group."

Locality.—The figured specimen is from Victoria Gully, Castlemaine.

Clonograptus rigidus, J. Hall, var. *tenellus*, Lns. (Pl. XXII, Fig. 8.)

The figured specimen shows a common form in the Lancefield beds, where it is associated with the typical form, as described by Hall. *C. rigidus* and *C. tenellus* cannot, I think, be separated, and the extremes are connected by a long intermediate series at Lancefield.

Goniograptus macer, n.sp. (Illustration, p. 450, Figs. 9, 10.)

Syn. *Loganograptus Logani*? R. Etheridge, jun. (*non* J. Hall): Ann. Mag. Nat. Hist., 1874, p. 4, pl. iii, fig. 12.

Id., McCoy (*non* J. Hall): Prod. Pal. Victoria, Dec. i, p. 19 (loc. B *b*, 29, specimens in the National Museum, Melbourne).

Goniograptus, sp., T. S. Hall: Proc. Roy. Soc. Victoria, N.S., vol. vii, 1894 (1895), p. 72.

Hydrosome slender. Primary branches about 1 mm. in length, and forming an angle of 180° with each other. Secondary branches diverging at about 90° from each other, and then bending in a zigzag manner at intervals of about 1·5 mm., and giving off tertiary branches from the salient angle. Tertiary branches form two to four in number. After giving off the final tertiary branches the secondary branches as well as the tertiaries may reach the length of 30 or 40 mm., and are fairly rigid. Sicula about 1 mm. in length, slender, and very slowly tapering. Thecæ eight or nine in 10 mm., overlapping by half their length. Apertural margin slightly concave, forming an angle of about 110° with the axis of the branch. Outer margin inclined at about 25° to the axis of the branch, gently curving towards the distal extremity.

Though the species is fairly common, the delicate nature of the hydrosome rarely allows of its being found in a well-preserved condition, and the thecæ are usually invisible in specimens in slate.

Under the name of *Goniograptus*, sp., I have previously dealt with this species, and shown that it has been confounded with *Loganograptus*, which occurs only in higher beds, the two forms never being associated. The species is evidently congeneric with *Dichograptus Kjerulfi*, Herrmann.[1]

[1] See Geol. Mag., 1886, pp. 13 et seq.

In the above description I have regarded the secondary branches as giving off tertiaries on alternate sides, instead of treating it as a case of dichotomy, that is, I look at it as showing what botanists term sympodial branching. In this I follow the view of McCoy in his description of the type species, *Goniograptus Thureaui*. In a simple species, such as the present, the distinction may appear trivial, but in the type species, and still more so in an undescribed member of the genus in the National Museum, Melbourne, simplicity is certainly gained by this manner of considering the branching. A similar mode of branching occurs, of course, in *Thamnograptus*.

Locality and Horizon.—It occurs throughout the Bendigo and in the lower members of the Castlemaine Series. In the former it is associated with the stouter and much more copiously branched *G. Thureaui*, which is confined to these beds.

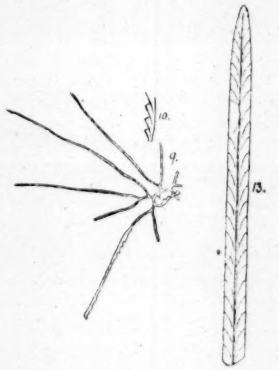


FIG. 9.—*Goniograptus macer*, n.sp.
,, 10.—The same, thecæ enlarged.
,, 13.—*Trigonograptus Wilkinsoni*, n.sp.
(Reduced to one-fourth nat. size.)

Bryograptus Victoriæ, T. S. Hall. (Pl. XXII, Figs. 11, 12.)

This species is fairly common at Lancefield, and indistinct specimens, with more branches developed than I have figured, occur, but I have shown a well-preserved form. It occurs on the same slabs with the *Leptograptus* figured above, as well as with most, if not all, of the species recorded from that locality in this paper.

Trigonograptus Wilkinsoni, n.sp. (Illustration supra, Fig. 13.)

Hydrosome straight, parallel-sided, gently curving to a blunt point at the distal end. Thecæ inclined at about 30°, gently curving outwards to the margin of the polypary, which they do not indent; about six and a half in 10 mm., opposite to one another. Virgula very distinct, straight, not traceable quite to the distal extremity. Breadth of hydrosome nearly 3 mm., greatest observed length (broken) 4 cm.

The species seems very distinct from those already described by the small number of thecæ in a given length. In general form it

approaches closely to Hall's figures of *T. ensiformis*, but the thecæ are opposite, and not alternate, and are gently curved. One specimen shows the proximal, or sicular end, to be more abruptly rounded than the distal.

Locality.—Sutherland's Creek, Darriwill, close to the spot marked "W.L.S.I." on Wilkinson's map.

DESCRIPTION OF PLATE XXII.

FIG. 1.—*Monograptus*, sp., enlarged.
,, 2.—*Didymograptus gracilis*, Tqt., enlarged.
,, 3.—*Leptograptus antiquus*, T. S. Hall.
,, 4.—The same, showing thecæ enlarged.
,, 5.—*Tetragraptus projectus*, n.sp.
,, 6.—The same, showing thecæ enlarged.
,, 7.—*Dichograptus octonarius*, J. Hall; thecæ enlarged.
,, 8.—*Clonograptus rigidus*, J. Hall, var. *tenellus*, Lns.
,, 11.—*Bryograptus Victoriæ*, T. S. Hall.
,, 12.—The same, enlarged.

(Unless otherwise stated, the figures are of the natural size.)

III.—On a New Species of *Lepracanthus* from the Yorkshire Coal-measures.

By Edgar D. Wellburn, L.R.C.P. & S.E., F.R.I.P.H., F.G.S., etc.

Genus Lepracanthus, Owen.

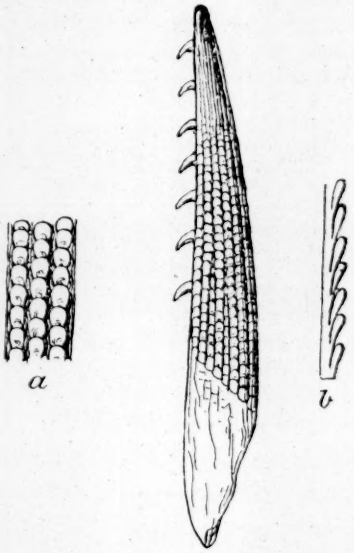
Geol. Mag., Dec. I, Vol. VI (1869), p. 481.

Lepracanthus rectus, sp. nov.

THE spine is 40 mm. in length, by 5 mm. in width at the junction of the inserted and exserted portions; from this point the spine gradually narrows to a moderately fine point at the apex. The posterior margin is nearly straight, the anterior one gently curved, the curve being more marked distally. The lateral surfaces meet in front at a moderately acute angle, the spine being much laterally compressed. The posterior surface is concave, and each margin is armed with the usual well-marked recurved denticles. The sides of the exserted portion are ornamented with a longitudinal series of ganoine-coated, tuberculated ridges. The tubercles are squarely shaped, and are arranged in a close series, even in the proximal portion of the spine. In the distal two-thirds there are five such ridges, but in the proximal third they are increased by intercalation to seven. Their direction is as follows:—The anterior one forms a prominent keel on the anterior border, the next two run to the tip more or less parallel to that border, one runs close to and parallel with the posterior border, another runs to the apex between these two sets, and, lastly, on each side of this ridge are intercalated two shorter ones. The inserted portion is well marked, and the line of demarcation between this and the exserted portion runs upwards and backwards, cutting the anterior and posterior margins at an angle of 45 degrees.

The present spine, for which I propose the specific name *rectus*, differs from *L. Colei*, Owen, in that it is straighter and more robust,

in the ridges being increased proximally by intercalation, and in the tubercles being squarely shaped and arranged in a close series, even in the proximal portion of the spine.



Lepracanthus rectus, sp. nov.—(*a*) Three ridges enlarged to show the ornamentation. (*b*) Ridge seen from the lateral aspect.

Type.—Author's collection.

Form. and Loc.—Shale above the Better Bed Coal, Lower Coal-measures, Low Moor, Yorks.

Both Dr. Traquair and Mr. A. Smith Woodward have seen the spine, and agree with me as to its specific distinction.

NOTICES OF MEMOIRS.

I.—British Association for the Advancement of Science. Dover, 1899.

Address to the Geological Section, by Sir Archibald Geikie, D.C.L., D.Sc., F.R.S., President of the Section.

AMONG the many questions of great theoretical importance which have engaged the attention of geologists, none has in late years awakened more interest or aroused livelier controversy than that which deals with Time as an element in geological history. The various schools which have successively arisen — Cataclysmal, Uniformitarian, and Evolutionist—have had each its own views as to the duration of their chronology, as well as to the operations of terrestrial energy. But though holding different opinions, they did not make these differences matter of special controversy among

themselves. About thirty years ago, however, they were startled by a bold irruption into their camp from the side of physics. They were then called on to reform their ways, which were declared to be flatly opposed to the teachings of natural philosophy. Since that period the discussion then started regarding the age of the Earth and the value of geological time has continued with varying animation. Evidence of the most multifarious kind has been brought forward, and arguments of widely different degrees of validity have been pressed into service both by geologists and palæontologists on one side and by physicists on the other. For the last year or two there has been a pause in the controversy, though no general agreement has been arrived at in regard to the matters in dispute. The present interval of comparative quietude seems favourable for a dispassionate review of the debate. I propose, therefore, to take, as perhaps a not inappropriate subject on which to address geologists upon a somewhat international occasion like this present meeting of the British Association at Dover, the question of Geological Time. In offering a brief history of the discussion, I gladly avail myself of the opportunity of enforcing one of the lessons which the discussion has impressed upon my own mind, and to point a moral which, as it seems to me, we geologists may take home to ourselves from a consideration of the whole question. There is, I think, a practical outcome which may be made to issue from the controversy in a combination of sympathy and co-operation among geologists all over the world. A lasting service will be rendered to our science if by well-concerted effort we can place geological dynamics and geological chronology on a broader and firmer basis of actual experiment and measurement than has yet been laid.

To understand aright the origin and progress of the dispute regarding the value of time in geological speculation, we must take note of the attitude maintained towards this subject by some of the early fathers of the science. Among these pioneers none has left his mark more deeply graven on the foundations of modern geology than James Hutton. To him, more than to any other writer of his day, do we owe the doctrine of the high antiquity of our globe. No one before him had ever seen so clearly the abundant and impressive proofs of this remote antiquity recorded in the rocks of the earth's crust. In these rocks he traced the operation of the same slow and quiet processes which he observed to be at work at present in gradually transforming the face of the existing continents. When he stood face to face with the proofs of decay among the mountains, there seems to have arisen uppermost in his mind the thought of the immense succession of ages which these proofs revealed to him. His observant eye enabled him to see "the operations of the surface wasting the solid body of the globe, and to read the unmeasurable course of time that must have flowed during those amazing operations, which the vulgar do not see, and which the learned seem to see without wonder."[1] In contemplating the stupendous results

[1] "Theory of the Earth," vol. i, p. 108.

achieved by such apparently feeble forces, Hutton felt that one great objection he had to contend with in the reception of his theory, even by the scientific men of his day, lay in the inability or unwillingness of the human mind to admit such large demands as he made on the past. "What more can we require?" he asks in summing up his conclusions; and he answers the question in these memorable words: "What more can we require? Nothing but time. It is not any part of the process that will be disputed; but after allowing all the parts, the whole will be denied; and for what?—only because we are not disposed to allow that quantity of time which the ablution of so much wasted mountain might require."[1]

Far as Hutton could follow the succession of events registered in the rocky crust of the globe, he found himself baffled by the closing in around him of that dark abysm of time into which neither eye nor imagination seemed able to penetrate. He well knew that, behind and beyond the ages recorded in the oldest of the primitive rocks, there must have stretched a vast earlier time, of which no record met his view. He did not attempt to speculate beyond the limits of his evidence. "I do not pretend," he said, "to describe the beginning of things; I take things such as I find them at present, and from these I reason with regard to that which must have been."[2] In vain could he look, even among the oldest formations, for any sign of the infancy of the planet. He could only detect a repeated series of similar revolutions, the oldest of which was assuredly not the first in the terrestrial history, and he concluded, as "the result of this physical inquiry, that we find no vestige of a beginning, no prospect of an end."[3]

This conclusion from strictly geological evidence has been impugned from the side of physics, and, as further developed by Playfair, has been declared to be contradicted by the principles of natural philosophy. But if it be considered on the basis of the evidence on which it was originally propounded, it was absolutely true in Hutton's time and remains true to-day. That able reasoner never claimed that the earth has existed from all eternity, or that it will go on existing for ever. He admitted that it must have had a beginning, but he had been unable to find any vestige of that beginning in the structure of the planet itself. And notwithstanding all the multiplied researches of the century that has passed since the immortal "Theory of the Earth" was published, no relic of the first condition of our earth has been found. We have speculated much, indeed, on the subject, and our friends the physicists have speculated still more. Some of the speculations do not seem to me more philosophical than many of those of the older cosmogonists. As far as reliable evidence can be drawn from the rocks of the globe itself, we do not seem to be nearer the discovery of the beginning than Hutton was. The most ancient rocks that can be reached are demonstrably not the first-formed of all. They

[1] "Theory of the Earth," vol. ii, p. 329.
[2] Op. cit., vol i, p. 173, note.
[3] Op. cit., vol. i, p. 200.

were preceded by others which we know must have existed, though no vestige of them may remain.

It may be further asserted that, while it was Hutton who first impressed on modern geology the conviction that for the adequate comprehension of the past history of the earth vast periods of time must be admitted to have elapsed, our debt of obligation to him is increased by the genius with which he linked the passage of these vast periods with the present economy of nature. He first realized the influence of time as a factor in geological dynamics, and first taught the efficacy of the quiet and unobtrusive forces of nature. His predecessors and contemporaries were never tired of invoking the more vigorous manifestations of terrestrial energy. They saw in the composition of the land and in the structure of mountains and valleys memorials of numberless convulsions and cataclysms. In Hutton's philosophy, however, "it is the little causes, long continued, which are considered as bringing about the greatest changes of the earth." [1]

And yet, unlike many of those who derived their inspiration from his teaching, but pushed his tenets to extremes which he doubtless never anticipated, he did not look upon time as a kind of scientific fetich, the invocation of which would endow with efficacy even the most trifling phenomena. As if he had foreseen the use that might be made of his doctrine, he uttered this remarkable warning: "With regard to the effect of time, though the continuance of time may do much in those operations which are extremely slow, where no change, to our observation, had appeared to take place, yet, where it is not in the nature of things to produce the change in question, the unlimited course of time would be no more effectual than the moment by which we measure events in our observations." [2]

We thus see that in the philosophy of Hutton, out of which so much of modern geology has been developed, the vastness of the antiquity of the globe was deduced from the structure of the terrestrial crust and the slow rate of action of the forces by which the surface of the crust is observed to be modified. But no attempt was made by him to measure that antiquity by any of the chronological standards of human contrivance. He was content to realize for himself and to impress upon others that the history of the earth could not be understood, save by the admission that it occupied prolonged though indeterminate ages in its accomplishment. And assuredly no part of his teaching has been more amply sustained by the subsequent progress of research.

Playfair, from whose admirable "Illustrations of the Huttonian Theory" most geologists have derived all that they know directly of that theory, went a little further than his friend and master in dealing with the age of the earth. Not restricting himself, as Hutton did, to the testimony of the rocks, which showed neither vestige of a beginning nor prospect of an end, he called in the evidence of the cosmos outside the limits of our planet, and declared that in

[1] "Theory of the Earth," vol. ii, p. 205.

[2] Op. cit., vol. i, p. 44.

the firmament also no mark could be discovered of the commencement or termination of the present order, no symptom of infancy or old age, nor any sign by which the future or past duration of the universe might be estimated.[1] He thus advanced beyond the strictly geological bases of reasoning, and committed himself to statements which, like some made also by Hutton, seem to have been suggested by certain deductions of the French mathematicians of his day regarding the stability of the planetary motions. His statements have been disproved by modern physics; distinct evidence, both from the earth and the cosmos, has been brought forward of progress from a beginning which can be conceived, through successive stages to an end which can be foreseen. But the disproof leaves Hutton's doctrine about the vastness of geological time exactly where it was. Surely it was no abuse of language to speak of periods as being vast which can only be expressed in millions of years.

It is easy to understand how the Uniformitarian school, which sprang from the teaching of Hutton and Playfair, came to believe that the whole of eternity was at the disposal of geologists. In popular estimation, as the ancient science of astronomy was that of infinite distance, so the modern study of geology was the science of infinite time. It must be frankly conceded that geologists, believing themselves unfettered by any limits to their chronology, made ample use of their imagined liberty. Many of them, following the lead of Lyell, to whose writings in other respects modern geology owes so deep a debt of gratitude, became utterly reckless in their demands for time, demands which even the requirements of their own science, if they had adequately realized them, did not warrant. The older geologists had not attempted to express their vast periods in terms of years. The indefiniteness of their language fitly denoted the absence of any ascertainable limits to the successive ages with which they had to deal. And until some evidence should be discovered whereby these limits might be fixed and measured by human standards, no reproach could justly be brought against the geological terminology. It was far more philosophical to be content, in the meanwhile, with indeterminate expressions, than from data of the weakest or most speculative kind to attempt to measure geological periods by a chronology of years or centuries.

In the year 1862 a wholly new light was thrown on the question of the age of our globe and the duration of geological time by the remarkable paper on the Secular Cooling of the Earth communicated by Lord Kelvin (then Sir William Thomson) to the Royal Society of Edinburgh.[2] In this memoir he first developed his now well-known argument from the observed rate of increase of temperature downwards from the surface of the land. He astonished geologists by announcing to them that some definite limits to the age of our planet might be ascertained, and by declaring his belief that this age must be more than 20 millions, but less than 400 millions of years.

[1] "Illustrations of the Huttonian Theory," § 118.
[2] Trans. Roy. Soc. Edin., vol. xxiii (1862).

Nearly four years later he emphasized his dissent from what he considered to be the current geological opinions of the day by repeating the same argument in a more pointedly antagonistic form in a paper of only a few sentences, entitled "The Doctrine of Uniformity in Geology briefly refuted."[1]

Again, after a further lapse of about two years, when, as President of the Geological Society of Glasgow, it became his duty to give an address, he returned to the same topic and arraigned more boldly and explicitly than ever the geology of the time. He then declared that "a great reform in geological speculation seems now to have become necessary," and he went so far as to affirm that "it is quite certain that a great mistake has been made—that British popular geology at the present time is in direct opposition to the principles of natural philosophy."[2] In pressing once more the original argument derived from the downward increase of terrestrial temperature, he now reinforced it by two further arguments, the one based on the retardation of the earth's angular velocity by tidal friction, the other on the limitation of the age of the sun.

These three lines of attack remain still those along which the assault from physics is delivered against the strongholds of geology. Lord Kelvin has repeatedly returned to the charge since 1868, his latest contribution to the controversy having been pronounced two years ago.[3] While his physical arguments remain the same, the limits of time which he deduces from them have been successively diminished. The original maximum of 400 millions of years has now been restricted by him to not much more than 20 millions, while Professor Tait grudgingly allows something less than 10 millions.[4]

Soon after the appearance of Lord Kelvin's indictment of modern geology in 1868, the defence of the science was taken up by Huxley, who happened at the time to be President of the Geological Society of London. In his own inimitably brilliant way, half seriously half playfully, this doughty combatant, with evident relish, tossed the physical arguments to and fro in the eyes of his geological brethren, as a barrister may flourish his brief before a sympathetic jury. He was willing to admit that "the rapidity of rotation of the earth *may* be diminishing, that the sun *may* be waxing dim, or that the earth itself *may* be cooling." But he went on to add his suspicion that "most of us are Gallios, 'who care for none of these things,' being of opinion that, true or fictitious, they have made no practical difference to the earth, during the period of which a record is preserved in stratified deposits."[5]

For the indifference which their advocate thus professed on their behalf most geologists believed that they had ample justification.

[1] Proc. Roy. Soc. Edin., vol. v, p. 512 (Dec. 18, 1865).
[2] Trans. Geol. Soc. Glasgow, vol. iii, pp. 1, 16 (February, 1868).
[3] "The Age of the Earth," being the Annual Address to the Victoria Institute, June 2, 1897: Phil. Mag., January, 1899, p. 66.
[4] "Recent Advances in Physical Science," p. 174.
[5] Presidential Address, Quart. Journ. Geol. Soc., 1869.

The limits within which the physicist would circumscribe the earth's history was so vague, yet so vast, that whether the time allowed were 400 millions or 100 millions of years did not seem to them greatly to matter. After all, it was not the time that chiefly interested them, but the grand succession of events which the time had witnessed. That succession had been established on observations so abundant and so precise that it could withstand attack from any quarter, and it had taken as firm and lasting a place among the solid achievements of science as could be claimed for any physical speculations whatsoever. Whether the time required for the transaction of this marvellous earth-history was some millions of years more or some millions of years less did not seem to the geologists to be a question on which their science stood in antagonism with the principles of natural philosophy, but one which the natural philosophers might be left to settle at their own good pleasure.

For myself, I may be permitted here to say that I have never shared this feeling of indifference and unconcern. As far back as the year 1868, only a month after Lord Kelvin's first presentation of his threefold argument in favour of limiting the age of the earth, I gave in my adhesion to the propriety of restricting the geological demands for time. I then showed that even the phenomena of denudation, which, from the time of Hutton downwards, had been most constantly and confidently appealed to in support of the inconceivably vast antiquity of our globe, might be accounted for, at the present rate of action, within such a period as 100 millions of years.[1] To my mind it has always seemed that whatever tends to give more precision to the chronology of the geologist, and helps him to a clearer conception of the antiquity with which he has to deal, ought to be welcomed by him as a valuable assistance in his inquiries. And I feel sure that this view of the matter has now become general among those engaged in geological research. Frank recognition is made of the influence which Lord Kelvin's persistent attacks have had upon our science. Geologists have been led by his criticisms to revise their chronology. They gratefully acknowledge that to him they owe the introduction of important new lines of investigation, which link the solution of the problems of geology with those of physics. They realize how much he has done to dissipate the former vague conceptions as to the duration of geological history, and even when they emphatically dissent from the greatly restricted bounds within which he would now limit that history, and when they declare their inability to perceive that any reform of their speculations in this subject is needful, or that their science has placed herself in opposition to the principles of physics, they none the less pay their sincere homage to one who has thrown over geology, as over so many other departments of natural knowledge, the clear light of a penetrating and original genius.

[1] Trans. Geol. Soc. Glasgow, vol. iii, p. 189 (March 26, 1868). Sir W. Thomson acknowledged my adhesion in his reply to Huxley's criticism. Op. cit., p. 221.

When Lord Kelvin first developed his strictures on modern geology he expressed his opposition in the most uncompromising language. In the short paper to which reference has already been made, he announced, without hesitation or palliation, that he "briefly refuted" the doctrine of Uniformitarianism which had been espoused and illustrated by Lyell and a long list of the ablest geologists of the day. The severity of his judgment of British geology was not more marked than was his unqualified reliance on his own methods and results. This confident assurance of a distinguished physicist, together with a formidable array of mathematical formulæ, produced its effect on some geologists and palæontologists who were not Gallios. Thus, even after Huxley's brilliant defence, Darwin could not conceal the deep impression which Lord Kelvin's arguments had made on his mind. In one letter he wrote that the proposed limitation of geological time was one of his "sorest troubles." In another, he pronounced the physicist himself to be "an odious spectre." [1]

The same self-confidence of assertion on the part of some, at least, of the disputants on the physical side has continued all through the controversy. Yet when we examine the three great physical arguments in themselves, we find them to rest on assumptions which, though certified as "probable" or "very sure," are nevertheless admittedly assumptions. The conclusions to which these assumptions lead must depend for their validity on the degree of approximation to the truth in the premises which are postulated.

Now it is interesting to observe that neither the assumptions nor the conclusions drawn from them have commanded universal assent even among physicists themselves. If they were as self-evident as they have been claimed to be, they should at least receive the loyal support of all those whose function it is to pursue and extend the applications of physics. It will be remembered, however, that thirteen years ago Professor George Darwin, who has so often shown his inherited sympathy in geological investigation, devoted his presidential address before the Mathematical Section of this Association to a review of the three famous physical arguments respecting the age of the earth. He summed up his judgment of them in the following words:—"In considering these three arguments I have adduced some reasons against the validity of the first [tidal friction]; and have endeavoured to show that there are elements of uncertainty surrounding the second [secular cooling of the earth]; nevertheless they undoubtedly constitute a contribution of the first importance to physical geology. Whilst, then, we may protest against the precision with which Professor Tait seeks to deduce results from them, we are fully justified in following Sir William Thomson, who says that 'the existing state of things on the earth, life on the earth—all geological history showing continuity of life, must be limited within some such period of past time as 100,000,000 years.'" [2]

More recently Professor Perry has entered the lists, from the

[1] Darwin's Life and Letters, vol. iii, pp. 115, 146.

[2] Rep. Brit. Assoc., 1886, p. 517.

physical side, to challenge the validity of the conclusions so confidently put forward in limitation of the age of the earth. He has boldly impugned each of the three physical arguments. That which is based on tidal retardation, following Mr. Maxwell Close and Professor Darwin, he dismisses as fallacious. In regard to the argument from the secular cooling of the earth, he contends that it is perfectly allowable to assume a much higher conductivity for the interior of the globe, and that this assumption would vastly increase our estimate of the age of the planet. As to the conclusions drawn from the history of the sun, he maintains that, on the one hand, the sun may have been repeatedly fed by infalling meteorites, and that on the other the earth, during former ages, may have had its heat retained by a dense atmospheric envelope. He thinks that "almost anything is possible as to the present internal state of the earth," and he concludes in these words: "To sum up we can find no published record of any lower maximum age of life on the earth, as calculated by physicists, than 400 millions of years. From the three physical arguments, Lord Kelvin's higher limits are 1,000, 400, and 500 million years. I have shown that we have reasons for believing that the age, from all these, may be very considerably underestimated. It is to be observed that if we exclude everything but the arguments from mere physics, the *probable* age of life on the earth is much less than any of the above estimates; but if the palæontologists have good reasons for demanding much greater times, I see nothing from the physicist's point of view which denies them four times the greatest of these estimates."[1]

This remarkable admission from a recognized authority on the physical side re-echoes and emphasizes the warning pronounced by Professor Darwin in the address already quoted—"at present our knowledge of a definite limit to geological time has so little precision that we should do wrong to summarily reject any theories which appear to demand longer periods of time than those which now appear allowable."[2]

This 'wrong' which Professor Darwin so seriously deprecated has been committed not once, but again and again, in the history of this discussion. Lord Kelvin has never taken any notice of the strong body of evidence adduced by geologists and palæontologists in favour of a much longer antiquity than he is now disposed to allow for the age of the earth. His own three physical arguments have been successively restated, with such corrections and modifications as he has found to be necessary, and no doubt further alterations are in store for them. He has cut off slice after slice from the allowance of time which at first he was prepared to grant for the evolution of geological history, his latest pronouncement being that "it was more than twenty and less than forty million years, and probably much nearer twenty than forty."[3] But in none

[1] Nature, vol. li, p. 585 (April 18, 1895).

[2] Rep. Brit. Assoc., 1886, p. 518.

[3] "The Age of the Earth," Presidential Address to the Victoria Institute for 1897, p. 10; also in Phil. Mag., January, 1899.

of his papers is there an admission that geology and palæontology, though they have again and again raised their voices in protest, have anything to say in the matter that is worthy of consideration.

It is difficult satisfactorily to carry on a discussion in which your opponent entirely ignores your arguments, while you have given the fullest attention to his. In the present instance geologists have most carefully listened to all that has been brought forward from the physical side. Impressed by the force of the physical reasoning, they no longer believe that they can make any demands they may please on past time. They have been willing to accept Lord Kelvin's original estimate of 100 millions of years as the period within which the history of life upon the planet must be comprised; while some of them have even sought in various ways to reduce that sum nearer to his lower limit. Yet there is undoubtedly a prevalent misgiving, whether in thus seeking to reconcile their requirements with the demands of the physicist they are not tying themselves down within limits of time which on any theory of evolution would have been insufficient for the development of the animal and vegetable kingdoms.

It is unnecessary to recapitulate before this Section of the British Association, even in briefest outline, the reasoning of geologists and palæontologists which leads them to conclude that the history recorded in the crust of the earth must have required for its transaction a much vaster period of time than that to which the physicists would now restrict it.[1] Let me merely remark that the reasoning is essentially based on observations of the present rate of geological and biological changes upon the earth's surface. It is not, of course, maintained that this rate has never varied in the past. But it is the only rate with which we are familiar, which we can watch and in some degree measure, and which, therefore, we can take as a guide towards the comprehension and interpretation of the past history of our planet.

It may be, and has often been, said that the present scale of geological and biological processes cannot be accepted as a reliable measure for the past. Starting from the postulate, which no one will dispute, that the total sum of terrestrial energy was once greater than it is now and has been steadily declining, the physicists have boldly asserted that all kinds of geological action must have been more vigorous and rapid during bygone ages than they are to-day; that volcanoes were more gigantic, earthquakes more frequent and destructive, mountain upthrows more stupendous, tides and waves more powerful, and commotions of the atmosphere more violent, with more ruinous tempests and heavier rainfall. Assertions of this kind are temptingly plausible and are easily made. But it is not enough that they should be made; they ought to be supported by some kind of evidence to show that they are founded on actual fact and not on

[1] The geological arguments are briefly given in my Presidential Address to the British Association at the Edinburgh Meeting of 1892. The biological arguments were well stated, and in some detail, by Professor Poulton in his Address to the Zoological Section of the Association at the Liverpool Meeting of 1896.

mere theoretical possibility. Such evidence, if it existed, could surely be produced. The chronicle of the earth's history, from a very early period down to the present time, has been legibly written within the sedimentary formations of the terrestrial crust. Let the appeal be made to that register. Does it lend any support to the affirmation that the geological processes are now feebler and slower than they used to be? If it does, the physicists, we might suppose, would gladly bring forward its evidence as irrefragable confirmation of the soundness of their contention. But the geologists have found no such confirmation. On the contrary, they have been unable to discover any indication that the rate of geological causation has ever, on the whole, greatly varied during the time which has elapsed since the deposition of the oldest stratified rocks. They do not assert that there has been no variation, that there have been no periods of greater activity, both hypogene and epigene. But they maintain that the demonstration of the existence of such periods has yet to be made. They most confidently affirm that whatever may have happened in the earliest ages, in the whole vast succession of sedimentary strata nothing yet has been detected which necessarily demands that more violent and rapid action which the physicists suppose to have been the order of nature during the past.

So far as the potent effects of prolonged denudation permit us to judge, the latest mountain upheavals were at least as stupendous as any of older date whereof the basal relics can yet be detected. They seem, indeed, to have been still more gigantic than those. It may be doubted, for example, whether among the vestiges that remain of Mesozoic or Palæozoic mountain-chains any instance can be found so colossal as those of Tertiary times, such as the Alps. No volcanic eruptions of the older geological periods can compare in extent or volume with those of Tertiary and recent date. The plication and dislocation of the terrestrial crust are proportionately as conspicuously displayed among the younger as among the older formations, though the latter, from their greater antiquity, have suffered during a longer time from the renewed disturbances of successive periods.

As regards evidence of greater violence in the surrounding envelopes of atmosphere and ocean, we seek for it in vain among the stratified rocks. Among the very oldest formations of these Islands, the Torridon Sandstone of North-West Scotland presents us with a picture of long-continued sedimentation, such as may be seen in progress now round the shores of many a mountain-girdled lake. In that venerable deposit, the enclosed pebbles are not mere angular blocks and chips, swept by a sudden flood or destructive tide from off the surface of the land, and huddled together in confused heaps over the floor of the sea. They have been rounded and polished by the quiet operation of running water, as stones are rounded and polished now in the channels of brooks or on the shores of lake and sea. They have been laid gently down above each other, layer over layer, with fine sand sifted in between them, and this deposition has taken place along shores which, though the waters that washed them

have long since disappeared, can still be followed for mile after mile across the mountains and glens of the North-West Highlands. So tranquil were these waters that their gentle currents and oscillations sufficed to ripple the sandy floor, to arrange the sediment in laminæ of current-bedding, and to separate the grains of sand according to their relative densities. We may even now trace the results of these operations in thin darker layers and streaks of magnetic iron, zircon, and other heavy minerals, which have been sorted out from the lighter quartz-grains, as layers of iron-sand may be seen sifted together by the tide along the upper margins of many of our sandy beaches at the present day.

In the same ancient formation there occur also various intercalations of fine muddy sediment, so regular in their thin alternations, and so like those of younger formations, that we cannot but hope and expect that they may eventually yield remains of organisms which, if found, would be the earliest traces of life in Europe.

It is thus abundantly manifest that even in the most ancient of the sedimentary registers of the earth's history, not only is there no evidence of colossal floods, tides, and denudation, but there is incontrovertible proof of continuous orderly deposition, such as may be witnessed to-day in any quarter of the globe. The same tale, with endless additional details, is told all through the stratified formations down to those which are in the course of accumulation at the present day.

Not less important than the stratigraphical is the palæontological evidence in favour of the general quietude of the geological processes in the past. The conclusions drawn from the nature and arrangement of the sediments are corroborated and much extended by the structure and manner of entombment of the enclosed organic remains. From the time of the very earliest fossiliferous formations there is nothing to show that either plants or animals have had to contend with physical conditions of environment different, on the whole, from those in which their successors now live. The oldest trees, so far as regards their outer form and internal structure, betoken an atmosphere neither more tempestuous nor obviously more impure than that of to-day. The earliest corals, sponges, crustaceans, mollusks, and arachnids were not more stoutly constructed than those of later times, and are found grouped together among the rocks as they lived and died, with no apparent indication that any violent commotion of the elements tried their strength when living or swept away their remains when dead.

But, undoubtedly, most impressive of all the palæontological data is the testimony borne by the grand succession of organic remains among the stratified rocks as to the vast duration of time required for their evolution. Professor Poulton has treated this branch of the subject with great fulness and ability. We do not know the present average rates of organic variation, but all the available evidence goes to indicate their extreme slowness. They may conceivably have been more rapid in the past, or they may have been

liable to fluctuations according to vicissitudes of environment.[1] But those who assert that the rate of biological evolution ever differed materially from what it may now be inferred to be, ought surely to bring forward something more than mere assertion in their support. In the meantime, the most philosophical course is undoubtedly followed by those biologists who in this matter rest their belief on their own experience among recent and fossil organisms.

So cogent do these geological and palæontological arguments appear to those at least who have taken the trouble to master them, that they are worthy of being employed, not in defence merely, but in attack. It seems to me that they may be used with effect in assailing the stronghold of speculation and assumption in which our physical friends have ensconced themselves and from which, with their feet, as they believe, planted well within the interior of the globe and their heads in the heart of the sun, they view with complete unconcern the efforts made by those who endeavour to gather the truth from the surface and crust of the earth. That portion of the records of terrestrial history which lies open to our investigation has been diligently studied in all parts of the world. A vast body of facts has been gathered together from this extended and combined research. The chronicle registered in the earth's crust, though not complete, is legible and consistent. From the latest to the earliest of its chapters the story is capable of clear and harmonious interpretation by a comparison of its pages with the present condition of things. We know infinitely more of the history of this earth than we do of the history of the sun. Are we, then, to be told that this knowledge, so patiently accumulated from innumerable observations and so laboriously co-ordinated and classified, is to be held of none account in comparison with the conclusions of physical science in regard to the history of the central luminary of our system? These conclusions are founded on assumptions which may or may not correspond with the truth. They have already undergone revision, and they may be still further modified as our slender knowledge of the sun, and of the details of its history, is increased by future investigation. In the meantime, we decline to accept them as a final pronouncement of science on the subject. We place over against them the evidence of geology and palæontology, and affirm that unless the deductions we draw from that evidence can be disproved, we are entitled to maintain them as entirely borne out by the testimony of the rocks.

Until, therefore, it can be shown that geologists and palæontologists have misinterpreted their records, they are surely well within their logical rights in claiming as much time for the history of this earth as the vast body of evidence accumulated by them demands. So far as I have been able to form an opinion,

[1] See an interesting and suggestive paper by Professor Le Conte on "Critical Periods in the History of the Earth": Bull. Dept. Geology, University of California, vol. i (1895), p. 313. Also one by Professor Chamberlin on "The Ulterior Basis of Time-divisions and the Classification of Geological History": Journal of Geology, vol. vi (1898), p. 449.

one hundred millions of years would suffice for that portion of the history which is registered in the stratified rocks of the crust. But if the palæontologists find such a period too narrow for their requirements, I can see no reason on the geological side why they should not be at liberty to enlarge it as far as they may find to be needful, for the evolution of organized existence on the globe. As I have already remarked, it is not the length of time which interests us so much as the determination of the relative chronology of the events which were transacted within that time. As to the general succession of these events, there can be no dispute. We have traced its stages from the bottom of the oldest rocks up to the surface of the present continents and the floor of the present seas. We know that these stages have followed each other in orderly advance, and that geological time, whatever limits may be assigned to it, has sufficed for the passage of the long stately procession.

We may, therefore, well leave the dispute about the age of the earth to the decision of the future. In so doing, however, I should be glad if we would carry away from it something of greater service to science than the consciousness of having striven our best in a barren controversy, wherein concession has all to be on one side and the selection of arguments entirely on the other. During these years of prolonged debate I have often been painfully conscious that in this subject, as in so many others throughout the geological domain, the want of accurate numerical data is a serious hindrance to the progress of our science. Heartily do I acknowledge that much has been done in the way of measurements and experiments for the purpose of providing a foundation for estimates and deductions. But infinitely more remains to be accomplished. The field of investigation is almost boundless, for there is hardly a department of geological dynamics over which it does not extend. The range of experimental geology must be widely enlarged, until every process susceptible of illustration or measurement by artificial means has been investigated. Field-observation needs to be supplemented where possible by instrumental determinations, so as to be made more precise and accurate, and more capable of furnishing reliable numerical statistics for practical as well as theoretical deductions.

The subject is too vast for adequate treatment here. But let me illustrate my meaning by selecting a few instances where the adoption of these more rigid methods of inquiry might powerfully assist us in dealing with the rates of geological processes and the value of geological time. Take, for example, the wide range of lines of investigation embraced under the head of Denudation. So voluminous a series of observations has been made in this subject, and so ample is the literature devoted to it, that no department of geology, it might be thought, has been more abundantly and successfully explored. Yet if we look through the pile of memoirs, articles, and books, we cannot but be struck with the predominant vagueness of their statements, and with the general absence of such numerical

data determined by accurate, systematic, and prolonged measurement as would alone furnish a satisfactory basis for computations of the rate at which denudation takes place. Some instrumental observations of the greatest value have indeed been made, but for the most part observations of this kind have been too meagre and desultory.

A little consideration will show that in all branches of the investigation of denudation opportunities present themselves on every side of testing, by accurate instrumental observation and measurement, the rate at which some of the most universal processes in the geological régime of our globe are carried on.

It has long been a commonplace of geology that the amount of the material removed in suspension and solution by rivers furnishes a clue to the rate of denudation of the regions drained by the rivers. But how unequal in value, and generally how insufficient in precision, are the observations on this topic! A few rivers have been more or less systematically examined, some widely varying results have been obtained from the observations, and while enough has been obtained to show the interest and importance of the method of research, no adequate supply of materials has been gathered for the purposes of accurate deduction and generalization. What we need is a carefully organized series of observations carried out on a uniform plan, over a sufficient number of years, not for one river only, but for all the important rivers of a country, and, indeed, for all the greater rivers of each continent. We ought to know as accurately as possible the extent of the drainage-area of each river, the relations of river-discharge to rainfall and to other meteorological as well as topographical conditions; the variation in the proportions of mechanical and chemical impurities in the river-water according to geological formations, form of the ground, season of the year, and climate. The whole geological régime of each river should be thoroughly studied. The admirable report of Messrs. Humphreys and Abbot on the "Physics and Hydraulics of the Mississippi," published in 1861, might well serve as a model for imitation, though these observers necessarily occupied themselves with some questions which are not specially geological and did not enter into others on which, as geologists, we should now gladly have further information.

Again, the action of Glaciers has still less been subjected to prolonged and systematic observation. The few data already obtained are so vague that we may be said to be still entirely ignorant of the rate at which glaciers are wearing down their channels and contributing to the denudation of the land.

The whole of this inquiry is eminently suitable for combined research. Each stream or glacier, or each well-marked section of one, might become the special inquiry of a single observer, who would soon develop a paternal interest in his valley and vie with his colleagues of other valleys in the fulness and accuracy of his records.

Nor is our information respecting the operations of the Sea much more precise. Even in an island like Great Britain, where the

waves and tides effect so much change within the space of a human lifetime, the estimates of the rate of advance or retreat of the shoreline are based for the most part on no accurate determinations. It is satisfactory to be able to announce that the Council of this Association has formed a Committee for the purpose of obtaining full and accurate information regarding alterations of our coasts, and that with the sanction of the Lords of the Admiralty the co-operation of the Coastguard throughout the three kingdoms has been secured. We may therefore hope to be eventually in possession of trustworthy statistics on this interesting subject.

The disintegration of the surface of the land by the combined agency of the Subaërial forces of decay is a problem which has been much studied, but in regard to whose varying rates of advance not much has been definitely ascertained. The meteorological conditions under which it takes place differ materially according to latitude and climate, and doubtless its progress is equally variable. An obvious and useful source of information in regard to atmospheric denudations is to be found in the decay of the material of buildings of which the time of erection is known, and in dated tombstones. Twenty years ago I called attention to the rate at which marble gives way in such a moist climate as ours, and cited the effects of subaërial waste as these can be measured on the monuments of our graveyards and cemeteries.[1] I would urge upon town geologists, and those in the country who have no opportunities of venturing far afield, that they may do good service by careful scrutiny of ancient buildings and monuments. In the churchyards they will find much to occupy and interest them, not, however, like Old Mortality, in repairing the tombstones, but in tracing the ravages of the weather upon them, and in obtaining definite measures of the rate of their decay.

The conditions under which subaërial disintegration is effected in arid climates, and the rate of its advance, are still less known, seeing that most of our information is derived from the chance observations of passing travellers. Yet this branch of the subject is not without importance in relation to the denudation not only of the existing terrestrial surface but of the lands of former periods, for there is evidence of more than one arid epoch in geological history. Here, again, a diligent examination of ancient buildings and monuments might afford some, at least, of the required data. In such a country as Egypt, for instance, it might eventually be possible to determine from a large series of observations what has been the average rate of surface-disintegration of the various kinds of stone employed in human constructions that have been freely exposed to the air for several thousand years.

Closely linked with the question of denudation is that of the Deposition of the material worn away from the surface of the land. The total amount of sediment laid down must equal the amount of material abstracted, save in so far as the soluble portions of that material are retained in solution in the sea. But we have still much to learn as to the conditions, and especially as to the rate of,

[1] Proc. Roy. Soc. Edin., vol. x (1879–80), p. 518.

sedimentation. Nor does there appear to be much hope of any considerable increase to our knowledge until the subject is taken up in earnest as one demanding and justifying a prolonged series of well-planned and carefully executed observations. We have yet to discover the different rates of deposit, under the varying conditions in which it is carried on in lakes, estuaries, and the sea. What, for instance, would be a fair average for the rate at which the lakes of each country of Europe are now being silted up? If this rate were ascertained, and if the amount of material already deposited in these basins were determined, we should be in possession of data for estimating not only the probable time when the lakes will disappear, but also the approximate date at which they came into existence.

But it is not merely in regard to epigene changes that further more extended and concerted observation is needed. Even among subterranean movements there are some which might be watched and recorded with far more care and continuity than have ever been attempted. The researches of Professor George Darwin and others have shown how constant are the tremors, minute but measurable, to which the crust of the earth is subject.[1] Do these phenomena indicate displacements of the crust, and, if so, what in the lapse of a century is their cumulative effect on the surface of the land?

More momentous in their consequences are the disturbances which traverse mountain-chains and find their most violent expression in shocks of earthquake. The effect of such shocks have been studied and recorded in many parts of the world, but their cause is still little understood. Are the disturbances due to a continuation of the same operation which at first gave birth to the mountains? Should they be regarded as symptoms of growth or of collapse? Are they accompanied with even the slightest amount of elevation or depression? We cannot tell. But these questions are probably susceptible of some more or less definite answer. It might be possible, for instance, to determine with extreme precision the heights above a given datum of various fixed points along such a chain as the Alps, and by a series of minutely accurate measurements to detect any upward or downward deviation from these heights. It is quite conceivable that throughout the whole historical period some deviation of this kind has been going on, though so slowly, or by such slight increments at each period of renewal, as to escape ordinary observation. We might thus learn whether, after an Alpine earthquake, an appreciable difference of level is anywhere discoverable, whether the Alps as a great mountain-chain are still growing or are now subsiding, and we might be able to ascertain the rate of the movement. Although changes of this nature may have been too slight during human experience to be ordinarily appreciable, their very insignificance seems to me to supply a strong reason why they should be sought for and carefully measured. They would not tell us, indeed, whether a mountain-chain was called into being in one gigantic convulsion, or was raised at wide intervals by successive uplifts,

[1] Report Brit. Assoc., 1882, p. 95.

or was slowly elevated by one prolonged and continuous movement. But they might furnish us with suggestive information as to the rate at which upheaval or depression of the terrestrial crust is now going on.

The vexed questions of the origin of Raised Beaches and Sunk Forests might in like manner be elucidated by well-devised measurements. It is astonishing upon what loose and unreliable evidence the elevation or depression of coast-lines has often been asserted. On shores where proofs of a recent change of level are observable it would not be difficult to establish by accurate observation whether any such movements are taking place now, and, if they are, to determine their rate. The old attempts of this kind along the coasts of Scandinavia might be resumed with far more precision and on a much more extended scale. Methods of instrumental research have been vastly improved since the days of Celsius and Linnæus. Mere eye-observations would not supply sufficiently accurate results. When the datum-line has been determined with rigorous accuracy, the minutest changes of level, such as would be wholly inappreciable to the senses, might be detected and recorded. If such a system of watch were maintained along coasts where there is reason to believe that some rise or fall of land is taking place, it would be possible to follow the progress of the movement and to determine its rate.

But I must not dwell longer on examples of the advantages which geology would gain from a far more general and systematic adoption of methods of experiment and measurement in elucidation of the problems of the science. I have referred to a few of those which have a more special bearing on the question of geological time, but it is obvious that the same methods might be extended into almost every branch of geological dynamics. While we gladly and gratefully recognize the large amount of admirable work that has already been done by the adoption of these practical methods, from the time of Hall, the founder of experimental geology, down to our own day, we cannot but feel that our very appreciation of the gain which the science has thus derived increases the desire to see the practice still further multiplied and extended. I am confident that it is in this direction more than in any other that the next great advances of geology are to be anticipated.

While much may be done by individual students, it is less to their single efforts than to the combined investigations of many fellow-workers that I look most hopefully for the accumulation of data towards the determination of the present rate of geological changes. I would, therefore, commend this subject to the geologists of this and other countries as one in which individual, national, and international co-operation might well be enlisted. We already possess an institution which seems well adapted to undertake and control an enterprise of the kind suggested. The International Geological Congress, which brings together our associates from all parts of the globe, would confer a lasting benefit on the science if it could organize a system of combined observation in any single one of the

departments of inquiry which I have indicated or in any other which might be selected. We need not at first be too ambitious. The simplest, easiest, and least costly series of observations might be chosen for a beginning. The work might be distributed among the different countries represented in the Congress. Each nation would be entirely free in its selection of subjects for investigation, and would have the stimulus of co-operation with other nations in its work. The Congress will hold its triennial gathering next year in Paris, and if such an organization of research as I have suggested could then be inaugurated, a great impetus would thereby be given to geological research, and France, again become the birthplace of another scientific movement, would acquire a fresh claim to the admiration and gratitude of geologists in every part of the globe.

II. — British Association for the Advancement of Science. Sixty-ninth Annual Meeting, held at Dover, September 14–20, 1899.

List of Papers read in Section C, Geology.

Sir Archibald Geikie, D.C.L., F.R.S., F.G.S., President.

Presidential Address by Sir A. Geikie (read 16th September).

R. Etheridge, F.R.S. — On the Relations between the Dover and Franco-Belgian Coal-Basins.

Professor W. Boyd Dawkins, F.R.S.—On the South-Eastern Coalfield.[1]

A. J. Jukes-Browne.—Note on a Boring through the Chalk and Gault near Dieppe.

Walcot Gibson.—Some recent Work among the Upper Carboniferous of North Staffordshire and its Bearings on concealed Coalfields.

E. Greenly.—Report of the Committee on the Drift Sections at Moel Tryfan.

C. B. Wedd.—Note on Barium Sulphate in the Bunter Sandstone of North Staffordshire.

Professor J. Milne, F.R.S.—Report of the Committee on Seismological Investigations.

Professor H. A. Miers, F.R.S.— Report of the Committee on the Structure of Crystals.

E. J. Garwood.—Report of the Committee on Life Zones in British Carboniferous Rocks.

Dr. A. W. Rowe.—The Photo-Micrography of Opaque Objects as applied to Delineation of the Minute Structure of Fossils. (Lantern.)

Dr. G. Abbott.—Water Zones: their Influence on the Situation and Growth of Concretions. (Lantern.)

——— Tubular and Concentric Concretions. (Lantern.)

E. Greenly.—On Photographs of Sandstone Pipes in the Carboniferous Limestone at Dwlbau Point, East Anglesey. (Lantern.)

——— Glaciation of Dwlbau Point, East Anglesey. (Lantern.)

P. F. Kendall.—Extra-Morainic Drainage in Yorkshire. (Lantern.)

[1] Professor W. Boyd Dawkins' diagrams of the new coal borings in Kent were on view in the Section Room.

J. Lomas.—On the Origin of Lateral Moraines and Rock Trains. (Lantern.)

Professor W. J. Sollas, F.R.S.—Note on the Origin of Flint. (Lantern.)

Dr. H. J. Johnston-Lavis.—The Structure of Oolite and Calcareous Confetti. (Lantern.)

Rev. G. C. H. Pollen.—Report of the Committee on the Ty Newydd Caves. (Lantern.)

Professor T. Rupert Jones, F.R.S.—Report of the Committee on Palæozoic Phyllopoda.

Professor W. J. Sollas, F.R.S.—On Homotaxy and Contemporaneity.

Professor W. W. Watts.—On the Rounded Surfaces of the Mount Sorrel Granite.

Professor A. Renard.—On the Origin of Chondritic Meteorites.

Captain McDakin.—On Coast Erosion. (Lantern.)

G. Dowker.—Coast Erosion. (Lantern.)

W. Whitaker, F.R.S.—Preliminary Report on Coast Erosion.

Vaughan Cornish.—On Photographs of Wave Phenomena. (Lantern.)

Dr. Tempest Anderson.—Note on the Eruption of Vesuvius in September, 1898. (Lantern.)

Professor G. Platania.—On Mount Etna and its recent Volcanic Phenomena.

Professor P. F. Kendall.—Investigation of the Underground Waters of Craven. Part I: The Sources of the Aire. (Lantern.)

Professor W. Boyd Dawkins, F.R.S.—On the Geology of the Channel Tunnel.

F. W. Harmer.—On a proposed New Classification of the Pliocene Deposits of the East of England.

——— The Meteorological Conditions of North-Western Europe during the Pliocene and Glacial Periods.

Professor W. W. Watts.—Report of the Committee on British Photographs of Geological Interest.[1]

Rev. J. M. Mello.—A note on some Palæolithic Implements of North Kent.

P. M. C. Kermode.—Report of Committee on Irish Elk Remains in the Isle of Man.

Professor A. P. Coleman.—Report of the Committee on the Flora and Fauna of the Interglacial Beds in Canada.

Mrs. Maria M. (Ogilvie) Gordon, D.Sc.—On Sigmoidal Curves in the Earth's Crust.

H. Bolton.—Report of the Committee on the Ossiferous Caves at Uphill.

Professor P. F. Kendall.—Report of the Committee on Erratic Blocks of the British Isles.

Dr. H. M. Ami.—On the Subdivisions of the Carboniferous System in certain portions of Nova Scotia.

A. Smith Woodward.—Report of the Committee on the Registration of Type Specimens.

[1] A series of geological photographs was exhibited by the Committee for the Collection and Preservation of Geological Photographs in the Committee Room.

III.—A Palæozoic Terrane beneath the Cambrian. By Geo. F. Matthew. [Annals New York Acad. Sci., vol. xii, No. 2, pp. 41–56.]

THIS article describes an unconformable series of rocks below the true Cambrian measures, containing *Paradoxides* and *Agraulos strenuus*, and therefore claimed to be pre-Cambrian. The series has been observed in Canada (Southern New Brunswick) and Newfoundland, in both of which countries the erosion of the underlying terrane (Etcheminian) to a greater or less extent had occurred before the deposition of the Cambrian.

Remains of a fauna had been found in these beds in New Brunswick, but only of a fragmentary kind, and only low organisms had, as a rule, been recognized. The Newfoundland beds yielded better results, and Mr. Matthew now records from the terrane the following forms:—

Hyolithes 2 species, *Orthotheca* 4 sp., *Urotheca* (n.gen.) 1 sp., *Aptychopsis* 1 sp., *Kutorgina* (?) 1 sp., *Obolella* 1 sp., *Obolus* 1 sp., *Coleoides* 1 sp., *Hyolithellus* 2 sp., *Helenia* 1 sp., *Palæaomœa* 1 sp., *Scenella* 2 sp., *Platyceras* 3 sp., *Modiolopsis* 1 sp., *Platysolenites* (?) 1 sp. Besides these there are fragments of Cystidians and burrows and trails of worms.

"The uniformity of conditions attending the deposition of the Etcheminian throughout the Atlantic Coast province of the Cambrian is surprising, and point to a quiescent period of long continuance, during which the Hyolithidæ and Capulidæ developed so as to become the dominant types of the animal world, while the Brachiopods, the Lamellibranchs, and the other Gasteropods still were puny and insignificant."

REVIEWS.

I.—The Survey Memoir on the Scottish Uplands.[1]

I HAVE been requested by my friend the Editor of the Geological Magazine to review this latest, finest, and most original of the monographs issued by the British Geological Survey; and although for many reasons I should have preferred that the request had been made to some younger geologist, and one less personally interested in the subject, yet as an old worker in the Upland region to which the volume is devoted, an opponent of the old ideas of the succession, and an advocate of the new, I have no choice but to comply.

In the first place attention must be directed to the great advance which this monograph shows upon the previous Survey publications of the corresponding type as regards external get-up, paper, printing, illustrations, and last, but by no means least, reasonableness in price. We have here a handsome volume, a large octavo of some

[1] Memoirs of the Geological Survey of the United Kingdom: "The Silurian Rocks of Britain," vol. i, Scotland, 1899.

750 pages, printed clearly upon good paper, illustrated by about 120 cuts of sections, sketch plans, and the like scattered through the letterpress, together with 18 full-page plate reproductions of excellent photographs of typical field-sections and field-structures, all printed in a manner to do them full justice; while in addition we have seven equally good full-page reproductions of microphotographic rock-structures collected at the end of the volume, two full-page plates of fossils, and a coloured index map and index sections of the region surveyed; and all for the modest price of 15*s*. It is to be hoped that now the Survey officers have thus demonstrated the possibility of issuing to the public a work in all these respects worthy of the Survey, they will insist that no future monograph shall fall below this standard.

For this is precisely as it should be. It is but reasonable, to say the least, that this national Survey, the current cost of which is borne by the people, should do its utmost to publish its results in the form most attractive and beneficial to the people, and at the lowest possible price; that the results of the Survey work—their maps and their memoirs—should be taken advantage of by as large a number of people as possible, not only for the sake of the advancement of the material prosperity of the country, but for the sake of the progress and influence of British geological knowledge at large.

To lovers of British geology it is mortifying to acknowledge how few persons, educated or uneducated, know, or care to know, anything of that science which of all sciences is the one pre-eminently British. And yet how naturally does every well-read geologist, not only British, but foreign, accept it as a trite and open secret that the primal source and foundation of the upward progress of our country, from the days of the great Elizabeth to those of the greater Victoria, has been the vast store of mineral wealth that Nature has placed around the homes and under the very feet of our people—a store of mineral wealth which, when contrasted with the insignificant size of the country, is equalled in no other land under the sun. Talk of the great landed nobles as we may, boast of the sturdy yeomen of the land and the hardy sailors of the seas, yet at the back of them all, back of all our national progress and prestige, lie the great coalfields and ironfields, that have founded and fed the great manufacturing districts. These in their turn have furnished employment and subsistence to our teeming populations, have brought wealth, leisure, influence to our middle classes; and have afforded to the nation at large the means of trade and intercommunication at home, of transport and commerce on the seas, and of conquest and colonization abroad.

And how thoroughly does every well-seasoned British geologist know that the vast extent and sway of our political British empire, upon which we boast that the sun never sets, is more than paralleled by the worldwide rule and dominance of the British empire of geological principles and ideas—an empire to which every geologist living takes by implication the oath of allegiance the day he

becomes a geologist. The host of the political British colonies and possessions scattered over the continents and the oceans, what are they to the host of British colonies and settlements scattered throughout the geological literature and practice of the entire geological world? Not a single map of any large district issued by the Geological Survey of any country, not a geological text-book published in any language, not a geological lecture delivered by any teacher, or a geological paper read by any amateur, from London to Tokio, from the St. Lawrence to the Plate, but shows how the geological nomenclature of every land has been colonized verbally by British settlers, who, in their stiff, self-sufficient, masterful way, enforce order and contentment among the aborigines around.

Of course, to the old geological hand all these things are but prosy platitudes—contemptible because they are so familiar. But who knows? Some day, perhaps, may arise a Kipling, who shall suddenly touch with them, burning, the hearts of the people, that they may open their eyes and see.

In the meantime, however, it is good to know that the science of geology is still advancing amongst us from discovery to discovery, that the amateur geologist is still one of the natural products of the British soil. It is pleasant also to note how the national Geological Survey issues year after year its maps and its memoirs, not only those dealing with areas well known of old to be conspicuous for their abundant mineral resources, or fruitful in geological interest, but also with areas which for the time being may seem to be but barren and desolate solitudes in both regards. And this, again, is but right. Who among the officers of those surveying ships of Britain, sent out a century or two ago to map the unknown coasts of America, Africa, and Australia, imagined even in their wildest dreams that the lonely lands they outlined would, in the no long distant future, teem with English-speaking populations, the pride of the mother country, and rich, powerful, and prosperous, for whose safe and certain advent these early surveys had prepared the road? Who, again, among British geologists, official or non-official, can take upon himself to say what hosts of new and important geological facts, rich in new deductions destined to advance the honour of British science, or to add to the wealth of the country, intellectual or material, may not follow from the careful mapping of apparently unpromising areas?

We have, indeed, a very good illustration of this in the case of the region described by the memoir we are reviewing. The Geological Surveyors, as in duty bound, practically deferred the complete mapping of this apparently unpromising region until all the neighbouring parts of Scotland, rich in materials of economic value, had been surveyed and mapped. Stage by stage, however, the Upland work has been taken up, until at last we have a complete outline picture of the structure and possibilities of the region. And in place of the older view of the unpromising nature of its geological formations they are found to be wonderfully rich in

geological facts and phenomena, needing for their explanation and classification a new race, so to speak, of unexpected geological conclusions and principles, destined to play an important part in the future of British geology.

In their original mapping of the Uplands, between 1854 and 1873, the officers of the Survey followed the familiar formational or regional plan of working out the succession, classing each broad geographical belt of strata having as a whole special and characteristic lithological features as the outcrop of a distinct rock group or formation, fixing its relative place in the general series of such formations by its visible superposition, and its approximate geological age by its collective fauna; a method which had proved itself in every case fully adequate for the ascertainment of the true sequence among the Lowland formations of Britain, and even among the tilted Silurian rocks of Wales, the North of England, Lanarkshire, and the Pentlands. Combining the work of all their predecessors with the results of their own researches, and interpreting the whole in this way, they concluded that the strata of the Uplands dipped off a grand anticlinal form ranging from Dumfries to St. Abbs, and that its highest formation, the geological age of which was fixed by its Caradoc fossils, lay in the lap of a synclinal form running through the Leadhills to the Moorfoots. Between the anticlinal line and the synclinal form occurred a series of seven rock-groups or subformations: (1) the axial or Ardwell beds, (2) the Moffat Black Shales, (3) the Queensberry Grits, (4) the Dalveen Group, (5) the Lowther Shales, (6) the Leadhills Black Shales, and (7) the Carsphairn Grit Group. These were all classed as being of Llandeilo age, for most of them afforded certain well-known Llandeilo graptolites. It was true the strata were often so folded that it was impossible to say whether we were ascending or descending in the order of the beds, but the general sequence appeared evident enough. It was true that with the Llandeilo graptolites were often intermixed forms known in so-called Caradoc and higher strata in other parts of Britain, but the well-known and generally accepted Theory of Colonies of the illustrious Barrande was supposed by many to afford a very simple and natural explanation of such anomalies.

But the facts ascertained and the results arrived at stage by stage by myself and others in the geology of this Upland region, more especially in the Moffat and Girvan districts (1872–1882), made it evident that the regional or formational plan of mapping these convoluted rocks of the Uplands was unreliable, and that the only method found invariably trustworthy was that of working by means of palæontological and lithological 'zones.' The host of evidences accumulated during the same period by geologists and palæontologists in Britain, Scandinavia, France, America, etc., that the Silurian Graptolite was as capable as the Jurassic Ammonite of being employed as a stratigraphical index, made it equally plain that the true sequence of the Silurian strata from end to end of the Uplands must of necessity be that of their zones of graptolites.

It was not, however, until 1888 that the officers of the Survey felt the necessity—or, to speak more accurately, found the time and the opportunity—for testing the general reliability of these zonal ideas in the Uplands as a whole, and of revising the geology in the full light of the new methods.

Looking back over the whole matter from our present standpoint, we see that there is little or no room for astonishment or regret at this delay. Although—and to their lasting honour be it said—the new ideas were from the first heartily welcomed by the Geological Societies, and adopted by the younger and more ardent geologists, they were long looked at askance by many of those geologists well qualified to judge of their value and their applicability.

Consider for a moment the demands the novel results and conclusions due to the zonal method made upon the common-sense and experience — I will not say credulity— of those British geologists who had spent many of the best years of their life face to face with Silurian rocks in the field, had often worked side by side with the great leaders in reducing them to order, and had the literature of the subject at their finger-ends.

The data upon which the older views of the Upland succession rested were those which naturally appealed to the experience of the true field geologist. They were first and foremost petrological. They were few in number, easy to obtain, and easy to interpret. The sequence followed, as it were, almost of itself, from the common-sense deductions which would be drawn by any ordinary geologist taking broad views of every-day field phenomena. The tectonic structure corresponding to this succession showed, even more conspicuously than usual, the deformed character of districts floored by the more ancient rock formations. And lastly, the simple theoretical picture this succession gave us of the physical conditions of the region in Silurian times showed us little or nothing that previous knowledge or opinion had not led us to expect.

But, on the other hand, the newer views of the succession were founded essentially upon palæontological considerations, and petrology was relegated to a place in the background. They were dependent upon the harmonizing of a host of minor and local details, always difficult and sometimes impossible to obtain; and the final results were confessedly, after all, first approximations to the truth. In the discussion of the bearing of these new results upon the Upland structure, the evidence of the senses was put out of court, and field experience and ordinary judgment flouted with paradoxes. Finally, when from all the results thus arrived at we attempted an explanatory picture of the physical conditions in Silurian time, the imagination seemed to run riot amid the unfamiliar and the unexpected.

If, in surveying these convoluted Upland strata, we restrict ourselves to the older and simpler regional or formational methods of mapping by broad mineralogical characters and visible superposition, we are forced to disbelieve in the individual graptolite species as a geological index. We are led quite naturally to the earlier Survey view that the visibly ascending sequence of Upland

lithological rock-bands is that of a geologically ascending succession of Silurian rock formations, embracing some eight distinct rock groups, all of which (except the highest) form, as shown by their *collective* graptolite fauna, a series of ascending stages in one general Upper Llandeilo series. The lowest formation of this Llandeilo series dips visibly off the great longitudinal anticlinal axis of Hawick and Dumfries, and its highest subsides below the core of the great Leadhills' synclinal form. Collectively, this series must be of enormous thickness, but is remarkably monotonous both as regards mineralogical character and fossils. And, judging from all these conclusions, it would appear that in Upper Llandeilo times this was a region of shallow and muddy seas, in which there was for ages little or no change in the physical conditions and little or no progress in the evolution of organic life.

If, on the contrary, we follow the newer and more complicated zonal methods of mapping the Uplands—in other words, by detailed criteria primarily palæontological—we soon find ourselves shut up to the conclusion that the graptolite species, instead of being, as was formerly believed, one of the most untrustworthy of geological witnesses, is in reality one upon whose testimony the stratigraphist can implicitly rely. We gather that the conclusions drawn from the evidences it affords accord perfectly with the detailed lithological evidences and the like; they allow us so to explain and correlate the ascertainable petrological and palæontological phenomena from end to end of the Upland region, that they afford us a clue to the true arrangement of its strata and the key to its structure.

We learn that the grand geographical rock-bands of the Uplands are not the outcrops of an ascending series of distinct rock-groups, but answer, broadly speaking, to those of the different lithological variations of strata laid down in the same geological period, as they slowly change in mineral character and in thickness when followed from the ancient Silurian shore-lines towards the depths of the seas.

We find that not only is the petrological distinctness of the various rock-bands no proof of their individuality, but that the relative thickness of such Upland formations as we can individualize has no relation to their relative geological importance. The insignificant group of the Moffat Shales, only some 300 feet in thickness, formerly classed as a mere subdivision of the Upland Llandeilo, is the representative in time of the three great Welsh formations, the Upper Llandeilo, the Bala, and the Llandovery, and, like them, is marked by three distinct faunas. On the other hand, the Upland Gala or Queensberry formation, 25 miles in breadth and many thousands of feet in thickness, must be looked upon merely as a magnified representative of the band of Tarannon Shale, which in Wales is only about 1,000 feet in thickness, and marks the transitional period of time between the Llandovery and the Wenlock.

But not only are many of the stratigraphical generalizations drawn from broad regional views of the strata of the Upland region thus superseded by others due to the application of the zonal methods, but also many of the tectonic conclusions drawn from broad views

of its apparently simple tectonic structure. Two examples must here suffice. The new reading of the succession interprets the widest visible 'synclinal' of the Leadhills as its broadest anticlinal form, and regards the 'anticlinal' of Dumfries and Hawick—the very backbone of the Uplands—merely as a squeezed-in synclinal of no stratigraphical importance whatsoever.

Nor would the theoretical picture of the physical conditions of the Upland region in Silurian times, drawn by any student of the zonal methods, bear much resemblance to that limned by anyone who had mapped the country on the old regional plan.

The detailed phenomena worked out by the zonal method call upon us, explicitly or implicitly, to believe that in Arenig and Llandeilo times where now in part the Scottish Uplands rise there lay a region of broad, deep, and clear seas, over which the slowly moving currents drifted floating fields of tangle and seaweed, from which, perhaps, hung suspended the plant-like graptolites. Into these clear seas came for ages little or no land-derived sediment, save now and again the dust from a distant volcano. Their deep waters were bordered to the north-west by islanded shores, at first crowned by active volcanoes, but afterwards buried up with many oscillations under massive sheets of pebbles, sand, and mud, often marvellously prolific in animal life. Slowly the floor of the deep sea became shallowed, and the shore-derived sediments travelled farther and farther seaward, each formation in the earlier part of the ascending series thus showing a gradual thickening in a landward direction, each formation in the later part showing an increased thickening as a whole. By the close of Silurian times the sea-floor had all emerged as solid land to face the wear and tear of rain and rivers, having in the process of time become wrinkled and folded in a most marvellous way. To-day, after eons of denudation, the old rock formations of Silurian times, thick and massive in the direction of the ancient shore-lines, thinning away almost to nothing where lay the deeps of the seas, still lie, each in contact, it is true, with its natural predecessor and successor in time; but so wrinkled and jammed together are they, so twisted and tangled locally, that only by studying each rock-sheet thread by thread, as it were, where it shows out now and again through the rents and the tatters of the sheet now above it, can we say with certainty which of those sheets it is we are studying, and what were the local circumstances of its deposition.

These are a few of perhaps the most striking contrasts between the results obtained by the interpretation of the phenomena presented by the convoluted rocks of the Uplands: on the one hand, by the older and simple *formational*, collective, or areal methods, as followed by Nicol, Sedgwick, Murchison, Harkness, and others, and the officers of the Survey up to the year 1873; and on the other hand, by those who have used, in whole or in part, the newer and more complicated *zonal* methods, as developed by British and foreign geologists, broadly speaking, between the years 1870 and 1886.

Before, therefore, any general memoir upon the Silurian rocks of

Britain could be issued by the Survey, it was evident that the old formational views of the Upland succession must first be rigidly tested in the field in the light of the more recent discoveries and opinions; and in the year 1888 Messrs. Peach and Horne, already long familiar with zonal methods in their brilliant work in the North-West Highlands, were deputed by the Director-General to make a complete revision of the Upland geology. During the years 1888–1899 they have from time to time been busied at this task. In the words of Sir Archibald Geikie in his preface to the book, "they have traversed, sometimes repeatedly, the various sections of the strata across the length and breadth of the Southern Uplands. They have mapped on the ground the successive rock-zones, traced the variation of these zones from one end of the region to the other, and made voluminous notes as regards their stratigraphy and palæontology."

The greater part of the present volume is the result of this revision, which, it is plainly evident, has been to all concerned a labour of love. CHAS. LAPWORTH.

(*To be concluded.*)

II.—APPLIED GEOLOGY. Part II. By J. V. ELSDEN, B.Sc., F.G.S. 8vo; pp. vi, 250. (London: "The Quarry" Publishing Co., 1899.)

THE first part of this work, a slim volume bound in cloth, was noticed in the GEOLOGICAL MAGAZINE for November, 1898 (p. 521). We then referred to the difficulties of the task which the author had undertaken. Evidently he has devoted great pains to his work, but, as might be expected, he cannot deal equally nor sufficiently fully with all sections of such a very comprehensive subject. As an introduction to the student and as an aid to the teacher, the book will no doubt be found useful; but there can be no question that the mining engineer, and those who require special information concerning building-stones, cements, water-supply, etc., must seek it in works dealing particularly with those subjects.

The author has gleaned facts from all parts of the world, and from a number of original sources, but he only occasionally acknowledges the information which he has utilized in the text, and in the accompanying illustrations. There is perhaps too great a tendency nowadays to regard published information (both text and figures) as common property, and to avoid burdening the text with references; but we cannot help thinking that the value of the present work would have been enhanced by references to works in which fuller information could be obtained. The illustrations (a few of which are acknowledged) have been reproduced in a bold diagrammatic style. A number of microscopic sections of rocks are also introduced, but on this subject, as on some others where special knowledge and training are essential, a little knowledge may be a dangerous thing.

The present work begins with chapter vi, thus continuing the subject-matter of Part I, but the paging is commenced anew. The author now deals with ore-deposits, building and ornamental stones,

and other rocks used in the arts and manufactures, with engineering geology and soils. In ascertaining the mode of occurrence and extent of various deposits of economic importance, a scientific knowledge of the structure of the ground is essential. In metalliferous regions it is needful to know the character of the plications as well as the nature of the rocks. The width of lodes may vary with rocks of different texture, and cavities filled with mineral matter may be expected at particular points in flexured rocks.

After giving some account of unstratified ore-deposits, the author briefly refers in chapter vii to peat, lignite, coal, and petroleum, and also to phosphates, rock-salt, gypsum, and cobalt. We cannot say that the author's treatment of his subject is systematic. In chapter viii we are told something about prospecting and developing, and the recognition of minerals, and also about quarrying and mining. Thence we pass on, in chapter ix, to building and ornamental stones. Here the author commences with a general account of igneous rocks, of dykes and sills and laccolites, and gives a short glossary of terms applied to rock-structures, a classification of igneous rocks, and a list of their chief rock-forming minerals. He then deals briefly with causes of weathering in building stones, a subject discussed subsequently in several chapters. In chapter xiii we have accounts of lithographic stone, lime and cement, brick and pottery clays, fire-clays, sands, grindstones, bath bricks, pigments, gems, and artificial stone. Fuller's earth is briefly mentioned, but that of Nutfield and Woburn should have been referred to Lower Greensand and not Lower Oolite.

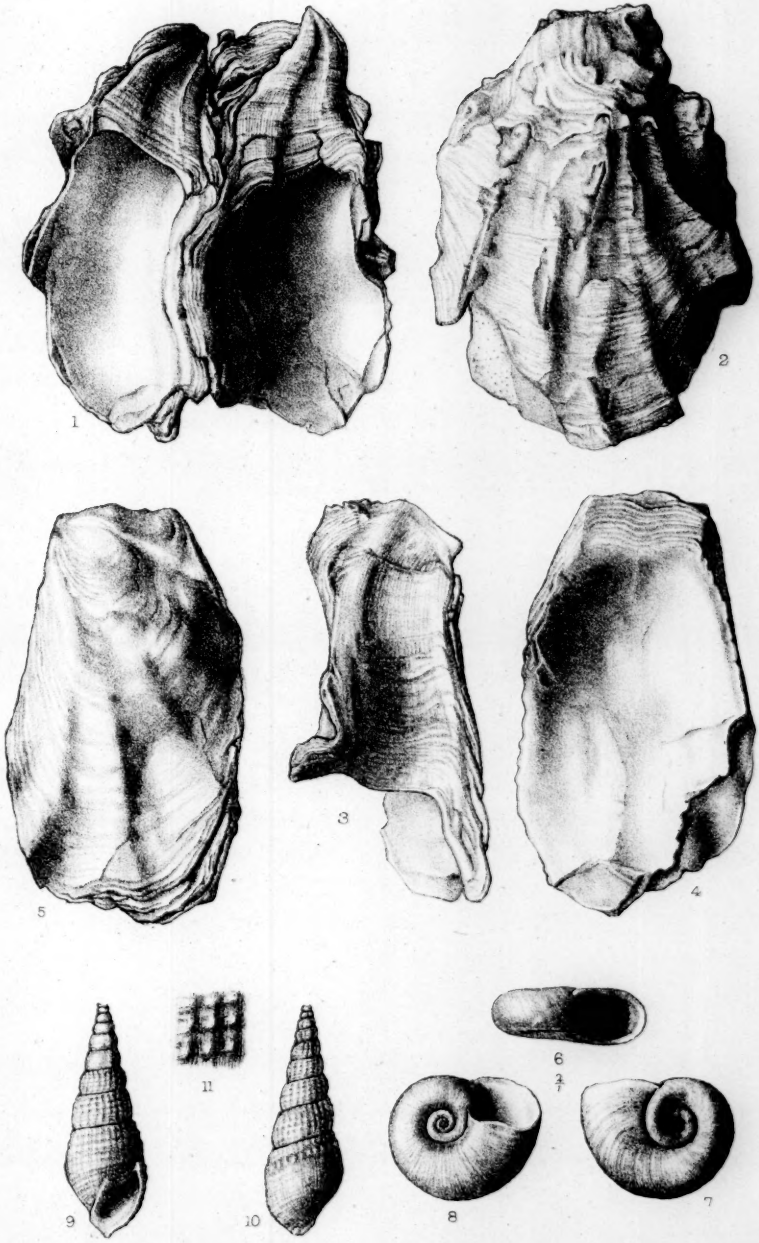
Chapter xiv contains an account of water-bearing strata and water-supply, with some useful diagrams, and two that are far from intelligible (figs. 147 and 149). Here the stratigraphical information is too meagre to be of much practical use. The references to impounding reservoirs in chapter xv are again too meagre to be of service. The concluding chapter deals with soils, and there is an appendix entitled "Simple rough methods for the determination of minerals and rocks." It would be well, as the author suggests, that the enquirer commences "by experimenting with known specimens."

CORRESPONDENCE.

GEOLOGICAL TIME.

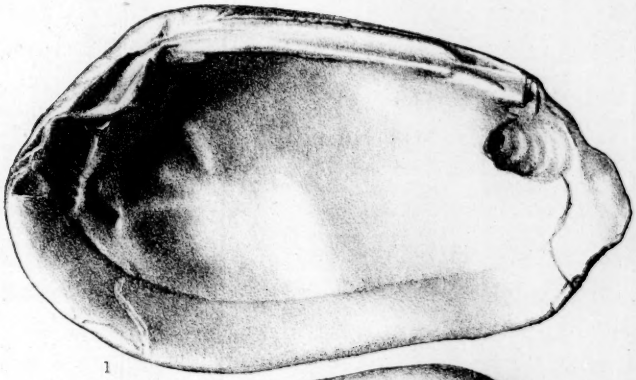
SIR,—All geologists will thank Sir A. Geikie for his admirable address to Section C at Dover this year. May I add another sentence bearing on "geological time," from Huxley's address in February, 1869 (Q.J.G.S., vol. xxv, p. L). He writes: "Mathematics may be compared to a mill of exquisite workmanship, which grinds you stuff of any degree of fineness; but nevertheless, what you get out depends on what you put in; and as the grandest mill in the world will not extract wheat-flour from peascods, so pages of formulæ will not get a definite result out of loose data."—F. G. S.

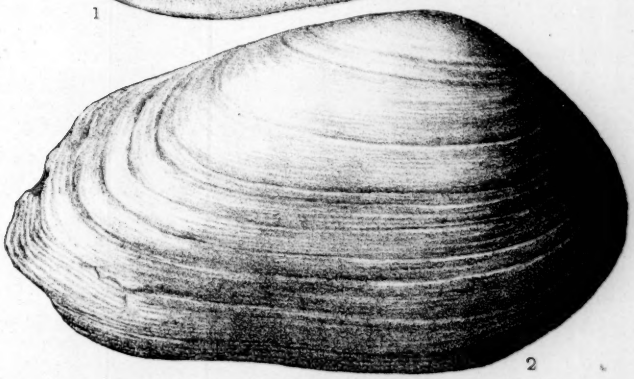
TURNHAM GREEN.

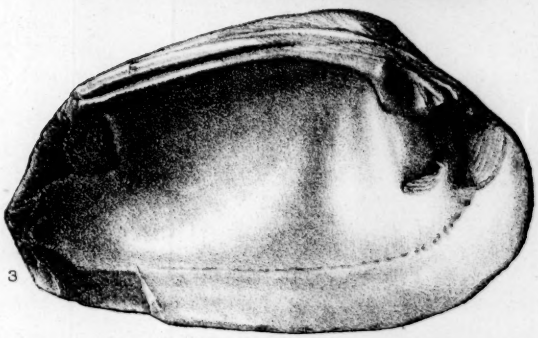


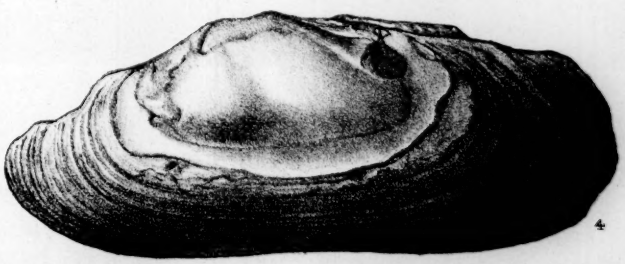
G.M.Woodward del. et lith. West, Newman imp.

Egyptian Pliocene and Post-Pliocene Shells.









G.M. Woodward del et lith. West, Newman imp.

Egyptian Post-Pliocene Shells.